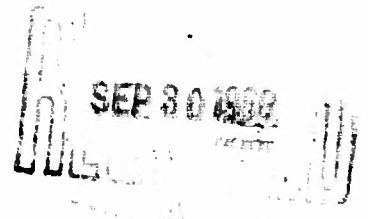AD 675148

AIR FORCE CAMBRIDGE RESEARCH LABORATORIES
L. G. HANSCOM FIELD, BEDFORD, MASSACHUSETTS

Proceedings of the Workshop on Methods of Obtaining Winds and Densities From Radar Meteor Trail Returns

Editors
ARNOLD A. BARNES, JR.
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Abstract

The first day of the four-day meeting held 16-19 August 1966 was devoted to technical descriptions of six radar meteor trail systems. Methods of deriving winds, wind shears, and geometric height of the trails were presented on the second day. Discussions of ambipolar diffusion rates and derived atmospheric densities and density-heights were the topics for the third day. On the last day the discussion centered around the use of the data by the meteorologist. The height resolution and data rates needed for climatological, tidal, and turbulence studies were delineated. Two papers on wind studies at Sheffield, England and at Adelaide, Australia were presented.

Preface

In the 1950's Manning, Eshleman, and Peterson at Stanford University laid the ground work for the measurement of winds from radar meteor trail returns. Further improvements were made on the technique by Greenhow at Jodrell Bank, England and by Elford and others at Adelaide, Australia. The AFCRL effort began in 1962 and was based on Greenhow's system. The first returns were seen in January 1964, and the first usable film data were taken on 6 March 1964. Contacts with other groups indicated that each group was working independently, and that there should be more cooperation and intercourse between the various groups. Representatives of the French group, the Stanford group, and the Adelaide group agreed that a meeting should be held and AFCRL agreed to sponsor the meeting. The announcement stated:

> The purpose of this meeting is to bring the atmospheric physicists,
> astronomers, and radio engineers together with the applied mete-
> orologist so that a better understanding of the meteorological prob-
> lems can be achieved and so that a unified attack on the unresolved
> problems can be mounted. Specifically, this meeting will bring
> together those <u>actively</u> <u>working</u> in this field so that we may better
> understand the accuracy of the height, wind, and density data
> obtained by various workers.

The first of the four days of the Workshop was devoted to descriptions of the six systems represented at the Workshop. This laid the ground work, set the stage for the discussions of the following days, and helped to point out some of the equipment problems.

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Determination and accuracy of the geometric height of the specular reflection point on the trails as well as the accuracy of the derived wind data were the topics for the second day. Because of the small scale atmospheric structure seen in rocket trails, the meteorologist would like position accuracy of a couple of hundred meters. Present height accuracy is about ± 2 kilometers, and the wind is a non-linear average over the first Fresnel zone which is roughly one kilometer in length.

Density, density height, and diffusion were discussed on the third day. The disagreement between theory and practice points up the fact that much more work will have to be done in this area. In particular, improved methods will have to be implemented to distinguish uniform, underdense trails capable of providing accurate density values.

On the last day of the Workshop the six stations agreed to take observations during the Geminids in December 1966 because of the uniform trails provided by this meteor shower. There was a lively discussion of the type and format of the data to be supplied to meteorologists and other users. Dr. Elford devised a table listing the required height accuracies and data rates needed for various wind studies. The final version, expressing the consensus of the Workshop, is included in the text.

Two meteorological papers were presented. Mr. Müller of Sheffield University spoke on "Atmospheric Tides in the Meteor Zone," and Dr. Elford's paper was on "Upper Atmospheric Wind Observations at Adelaide."

The meeting was held at the Charter House Motor Hotel, Winter Street exit, Route 128, Waltham, Mass., 16-19 August 1966. Attendance was limited to 40 people so as to allow the sessions to be informal and to maximize the exchange of information. A group photograph and list of attendees is included in the appendix. Stenographic records were taken and subsequently sent to the speakers so that the presentations could be revised for inclusion in these proceedings. The different responses from the speakers did not allow editing of the papers in a uniform manner. Hence some papers are abstracts, others edited stenographic notes and others finished, formal papers.

The editors would like to thank Helen Angier for typing the draft versions of the proceedings.

<div style="text-align: right;">
Arnold A. Barnes, Jr.

Joseph J. Pazniokas
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Contents

PROCEEDINGS OF THE WORKSHOP ON METHODS OF OBTAINING WINDS AND DENSITIES FROM RADAR METEOR TRAIL RETURNS

Welcoming Address

Dr. Morton L. Barad
Meteoralagy Laboratory
Air Farce Cambridge Research Laboratories
Bedford, Massachusetts

DR. BARNES. I would like to introduce the gentleman who has made this Workshop possible. It was through his foresight that the Upper Atmosphere Branch is, at this time, actively pursuing research to obtain useful, routine, meteorological data from meteor trail returns. He and Mr. Hering have successfully obtained funds so that we could continue our work, and, most important of all, he approved the expenditure of funds for this Workshop. For the past five years he has been Chief of the Meteorology Laboratory of the Air Force Cambridge Research Laboratories. I would like at this time to present to you Dr. Morton Barad.

DR. MORTON L. BARAD. Thank you. It is a pleasure to be here this morning and to be able to welcome all of you to this Workshop. I think that Arnold's idea to keep the membership or registration limited should serve to make this a more interesting meeting to all of you who have come from many parts of the globe to attend this meeting. As Arnold has indicated, we have been engaged with this problem for something like four years now. I think this is a good time for us at

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AFCRL to take stock to see where we stand. At the same time, we hope all of
you who have traveled from so far will also have an opportunity to find out what
others have done; in this way, the exchange of information will have been worth
the trip. It is a great pleasure for us to be able to sponsor this Workshop, and
we hope that in the course of the next few days you will find it worthwhile. I think
it would be profitable to move right into the program.

Workshop Chairman's Remarks

DR. BARNES. Thank you very much, Dr. Barad.

During the past three years, I have met with representatives from the installations at Adelaide, Australia; Stanford, California; Havana, Illinois; and France. The consensus was that those actually working in the field of wind and density measurements, obtained from radar meteor trail returns, should get together to exchange information with one another and to learn the problems which face the meteorologists who currently need such data for both applied and theoretical purposes.

It is obvious to everyone of us that we can obtain information for the meteorologists, but on the other hand, the meteorologists must know the limitations on the data so that they will not be misled when using the data. If we look at it from another point of view, the meteorologist should tell us what he knows about this region of the atmosphere, what he wants to know, and what sort of measurements with what accuracy are needed to answer his questions. I have tried to achieve these objectives in organizing this meeting.

Even though we have a formal program, I hope this can be an informal meeting; and I am sure that it can be with the reduced attendance that we have. Our first speaker is from the A. Weather Service. He is here to tell us why the wind and density data between 80 and 120 kilometers is needed and for what it will be used. Major Thomas Studer of the Air Weather Service.

Data User's Requirements

Major Thomas Studer
Air Weather Service

I should qualify Dr. Barnes' statement that I will speak for the users. What I have to say will indicate Air Force requirements, but I cannot speak for NASA or for ESSA. Their requirements, or at least NASA's requirements, appear very similar to ours.

The Air Force requirements for knowledge of winds and densities in the interval from 60 to 120 km arise both from a need to provide immediate operational support and from a longer-term requirement for a better climatology of the atmosphere for use in designing space vehicles, and so forth. The need for operational support of the original Dyna-soar program, and to a lesser degree of the Skybolt project, were early factors focusing military interest on ionized meteor trails as a source of information on densities and winds from these regions.

These programs have been cancelled and follow-on programs have not carried so great a need for wind information, but the urgent requirement for density data continues. Perhaps the most important of these current programs demanding density measurements are Apollo and Saturn. The Saturn vehicle, which should put a man on the moon by 1969 or 1970, has a stage separation near the altitude range of 80 to 100 km. This is a critical portion of the flight profile and system performance at this time is somewhat dependent on density. The PRIME program, aimed at developing a boost-glide vehicle capable of returning satellite payloads to a ground landing site, is similar in many ways to the older Dyna-soar program.

Vehicle reentry into the atmosphere will follow skip profile, undergoing a series of sinusoidal undulations in the region of 40 to 110 km, before deploying a parachute for descent to earth. The amount of lift imparted to the vehicle during this series of maneuvers and, therefore, the final landing site, depends on the ambient density.

The second requirement for this data, that of a general climatology for high altitude regions, has not received a great deal of emphasis. Interest in this area should grow in the future, however, especially as means of attaining accurate data are developed and improved. The Air Weather Service has an expressed requirement for a forecasting capability for high altitudes, and, obviously, this cannot be developed until more comprehensive measurements are made at these altitudes. To realistically design vehicles operating through the atmosphere, either going into space or returning from space, reasonably complete and accurate information describing the general daily and seasonal variations of the atmosphere, as well as probably extreme conditions to be expected, are required. Information of this kind from the lower atmosphere has been invaluable in the past for designing conventional airframes; it's only reasonable to presume it will be of great value in designing more exotic space vehicles.

It is difficult at the present time to estimate the density of data that will be needed. Oddly enough, more information must be acquired before this can be realistically estimated.

A thirteen-station rocketsonde network is now in existence gathering data to 200,000 ft. An expansion of this to include more systematic measurements in the Southern Hemisphere would be most desirable, but the costs involved in doing so are large. There is reason to believe that a greater number of systems will be needed to measure the environment at higher altitudes.

I am relatively new to this general area and am still acquiring a feel for the capabilities of systems that can sample the atmosphere at such high altitudes. Cost factors, to include installation and continuing maintenance expenses, are also features in which I'm interested. In short, listening to this discussion over the next two or three days should be most interesting.

Session 1

Description of Radar Meteor Trail Systems

Chairman

Robert F. Myers

Introductory Remarks

DR. BARNES. Thank you, Major Studer. The first session will be on the description of the various radar systems.

The Chairman is Mr. Robert F. Myers, who is the Senior Engineer, Meteorology Laboratory, Air Force Cambridge Research Laboratories.

ROBERT F. MYERS. What is there left for me to say? This first session on Descriptions of Systems will not answer Major Studer's problems of how much the systems cost or how much it will cost to maintain them, but this should give an appreciation of the system problems in an attack on the use of atmospheric meteor trails as meteorological sensors. I believe the real intent of today's meeting is one of education for us all. The first system we want to discuss is that of the University of Adelaide in Australia. The first speaker is Dr. W. Graham Elford.

I. The Adelaide Radio-Meteor System

Dr. W. Graham Elford
Department of Physics,
University of Adelaide,
Australia

Abstract

A 27-MHz combined cw and pulse radio system for comprehensive observations of meteor trails is described. The equipment affords measurements of systematic and turbulent wind and meteor orbits on a routine basis. The physical principles underlying these measurements are discussed and the main features of the equipment described.

1. INTRODUCTION

Measurements at Adelaide, South Australia, of the motion of the upper atmosphere by radio observations of drifting meteor trails commenced in 1952 using a combined continuous wave and pulse technique. Since that time the system has undergone many revisions but the basic principles behind the system at present in use are essentially the same as those put forward by Robertson, Liddy, and Elford (1953) when the first prototype system was described.

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In 1959 a network of remote receiving sites was added to the system in order that the orbits of individual meteors could be measured and small scale turbulence studied. The complete system thus afforded a unique combination of geophysical and astronomical measurements. A thirteen-month survey of systematic winds, turbulence, and meteor orbits was commenced in December 1960. These observations succeeded in revealing the presence of numerous weak meteor streams (Niisson, 1964) and a semi-annual variation in the rate of dissipation of turbulent energy at 80 to 100 km (Roper and Elford, 1963).

This paper describes the present equipment and the physical principles underlying the measurements. A brief description of a single station conerent radar system developed for measurements of meteor drifts at an Antarctic site is given in an Appendix.

2. THE AIMS OF THE PROJECT

The chief aims of the project may be summarized as follows:
(i)   The measurement of the rate of drift of meteor trails in order to study the diurnal and seasonal variations in the motion of the atmosphere between 75 and 105 km.
(ii) The measurement of the relative drift of three or more portions of individual meteor trails, for a study of atmospheric turbulence.
(iii) The measurement of the orbits of individual sporadic and shower meteors down to the 8th magnitude on a routine basis.
(iv) A statistical study of the distribution of ionization along meteor trails.
(v)   An examination of the dependence of the diffusion of meteor ionization on height and on the orientation of the trail with respect to the Earth's magnetic field.

Of these objectives only the first, the study of systematic winds, can be carried out with a single receiving site. The other four projects all depend on the successful triangulation of the flight paths of individual meteors for which at least two other receiving sites are required.

3. THE EQUIPMENT

The present equipment consists of two transmitters, continuous wave and pulse, at Adelaide, a main receiving station at St. Kilda, 22.9 km to the north of the transmitters, and three outstations. Data from the outstations are relayed by FM links to the main station where all the information is recorded. The station geometry is shown in Figure 1.
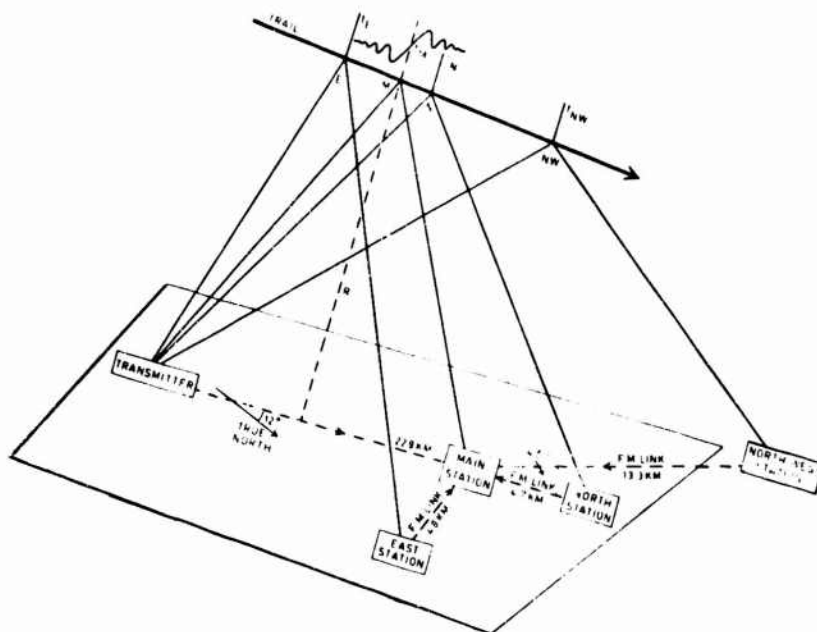
Figure 1. Schematic Diagram (not to scale) of the Locations of
the Stations and the Geometry of the Adelaide Radio Meteor Sys-
tem. Times t_E, t_M, t_N, t_{NW} are the times when the meteoroid
passes the reflection points E, M, N, NW for the four stations. A
typical diffraction pattern generated as the meteoroid passes a
specular reflection point is shown. All recording is done at the
main station.
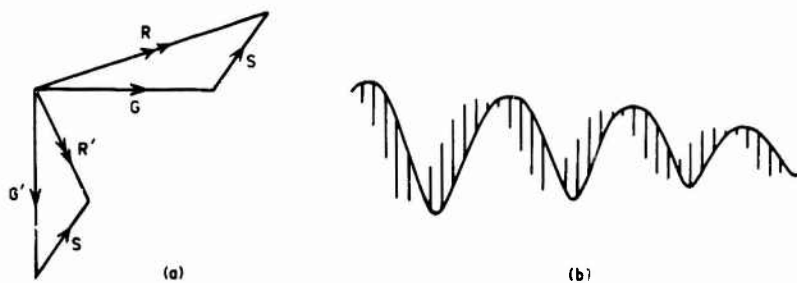
Figure 2. (a) Phase Diagram Snowing the Resultant Amplitude
at the Receiving Antennas in Terms of the Ground Wave G and
Sky Wave S. The vector triangle GSR is replaced by G'SR' for
the duration of the sense "spike"

(b) A Doppler Record. The sense spikes delineate a phantom
trace that either leads or lags the main trace by 90°

3.1 Techniques of Measurements

The quantities basic to the measurement of winds are the line-of-sight drift velocities of individual meteor trails and the location in space of the reflection points. The drift of the trail is obtained by measuring the rate of increase of the phase path of the sky wave, by comparing its phase with the (constant) phase of the ground wave at the receiving aerial; mixing is performed at the aerials. Alternatively this may be described as the measurement of the Doppler shift in frequency of the sky wave on its reflection at the moving meteor trail.

The sense of drift is determined by a periodic (50 Hz) saw-tooth phase modulation of the transmitted wave. The phase is retarded by 90° in 80 μsec and is then restored to zero in 20 msec. The slow advancement of phase is virtually synchronous in the ground wave and the sky wave at the receiving sites, but the rapid retardation occurs in the ground wave G before it does so in the sky wave S because of the path differences. Thus for a period of 500 to 1000 μsecs every 1/50 sec the ground wave G is retarded in phase by 90° but the phase of S is unchanged, as shown in Figure 2(a). During this interval the vector triangle GSR is replaced by G'SR' and the resultant amplitude at the receiver changes from R to R'. On the Doppler record, Figure 2(b), the amplitude R' appears as "spikes" which delineate a phantom trace shifted in phase by 90° with respect to the main waveform. The sense of the shift depends on the sense of rotation of S, i.e., according as the phase path is increasing or decreasing.

The direction of the echoing point is found by a comparison of the phases of the sky waves at five spaced receiving antennas at the main station. The layout of the antenna system is shown in Figure 3. The directions OE and ON define the positive axes of a cartesian coordinate system with respect to which the direction of the sky wave is defined by the direction cosines ℓ and m. The transmitter lies on the line OS. If coupling between antennas is neglected* the ground wave excites currents in the five antennas with the following phases (relative to antenna one).

Antenna	Phase (radians)
1	0
2	0
3	0
4	$\pi/2$
5	$3\pi/2$

* Analysis has shown that the errors introduced by coupling between antennas 1, 2, and 3 are less than the errors associated with reading the film records. Coupling between antennas 4 and 5 is significant but the signals from these antennas are used only to resolve ambiguities in direction.
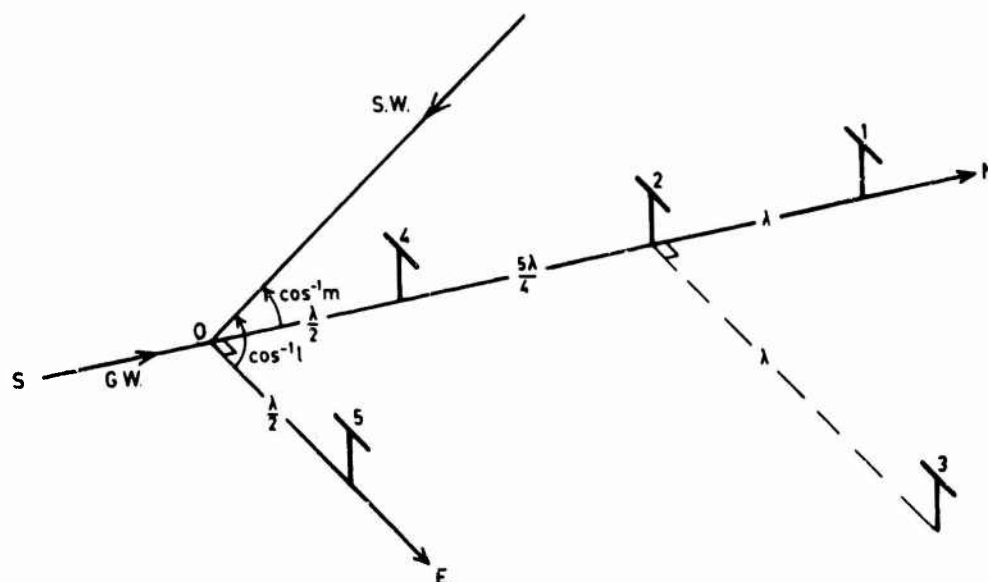
Figure 3. Layout of the Antennas Used for Determining the Direction of Arrival of the Sky Wave. The transmitter lies on the line OS

Fortunately the relative rf phases of the sky waves are preserved in the relative phases of the Doppler beat signals at the respective antennas, and thus the usual precautions for comparison of rf phases are unnecessary. An analysis of phase diagrams for each antenna shows that, for the case when the total sky wave phase path is decreasing (reflection point approaching) the resultant Doppler beat signals R_{1-5} have the following phase relationships:

R_1 leads R_2 by $2\pi m$ radians,

R_3 leads R_2 by $2\pi \ell$ radians,

R_4 leads R_2 by $2\pi(\frac{3}{4} - \frac{5m}{4})$ radians,

R_5 leads R_2 by $2\pi(\frac{1}{4} + \frac{\ell}{2} - \frac{7m}{4})$ radians.

These four measurements of relative phase are sufficient to determine unambiguously the direction of arrival of the sky wave. If the sky wave phase path is increasing (reflection point receding) the values of ℓ and m are multiplied by -1.0.

In theory the sense of the reflection point motion can also be obtained from these observations but in practise the effect of coupling between antennas 4 and 5 reduces the reliability of this means of determining sense. However, this redundancy is of value in checking the internal consistency of the film reading.

The range from the receiving site is determined by measuring the time difference between the arrival of a ground wave pulse and the associated sky wave pulse and making an appropriate correction for the separation of the transmitting and the receiving sites.

For turbulence studies and orbit measurements the orientation of the flight path and the velocity of the meteoroid are required. Consideration of the reflection process as a diffraction problem indicates that so long as the initially straight meteor trail is not distorted the reflection is specular, i.e., an echo is received from the trail only when the perpendicular to the trail passes through the observing station (more strictly, when the trail is tangent to an ellipsoid with transmitter and receiver sites as foci). Furthermore, most of the echo power is returned from a short segment of trail, of the order of one Fresnel zone in length, centered on the specular reflection point. At 27 MHz and for a typical range of 100 km, the length of this segment is approximately 0.7 km. In general, this will be smaller than the separation of the specular reflection points for the receiving stations.

The orientation of the flight path is determined from observations of the times of passage of the meteor particle (more strictly, the advancing head of the ionized column) past the specular reflection points appropriate to each receiving station (Figure 1). In practise, these times of passage are determined from an analysis of the diffraction wave-forms observed at each receiving site. From the relative times of passage and the geometry of the system the spatial separation of the reflection points can be calculated. If a steady wind or wind shear is present the meteor trail will not lie parallel to the path of the meteoroid but from a knowledge of the line of sight motion of the reflection points an accurate correction can be applied.

The velocity of the meteoroid is determined by measuring the time between successive maxima and minima of the diffraction waveform. In the cw case these measurements are complicated by the presence of the ground wave, and also by the slow changes in the phase of the sky wave, which produces the body Doppler waveform. However, these complications are more than compensated by the fact that in the cw case the effect of diffusion of the trail on the diffraction waveform is insignificant.

Other parameters measured are the ambipolar diffusion coefficient, inferred from the rate of decay of the amplitude of the echo, and the peak echo amplitude. The echo amplitude, taken in conjunction with the equipment parameters and the antenna polar diagrams, gives the line density of electrons at the reflection point. Thus these two parameters and the line-of-sight drift of the trail can be determined at four points whose relative positions are accurately known.

3.2 Description of the Equipment

3.2.1 TRANSMITTERS

The cw transmitter operates on a frequency of 26.773 MHz and the pulse transmitter on a frequency of 27.540 MHz. Each transmitter feeds a simple half-wave dipole, one quarter wave above ground. Block diagrams of each transmitter are given in Figure 4.

RADAR TRANSMITTER
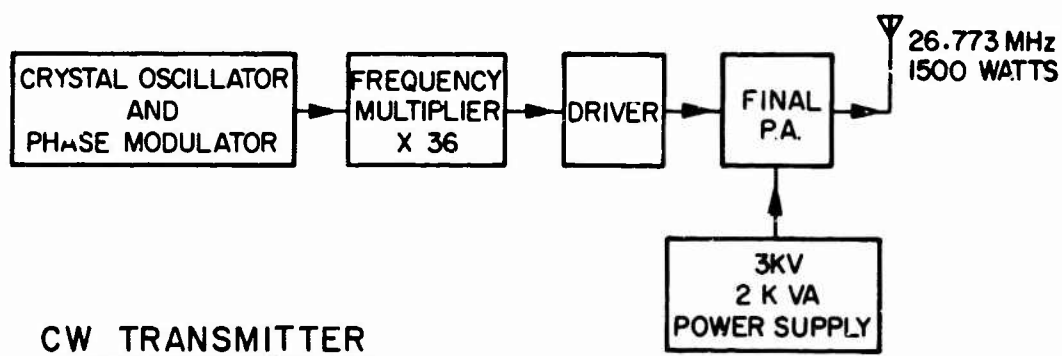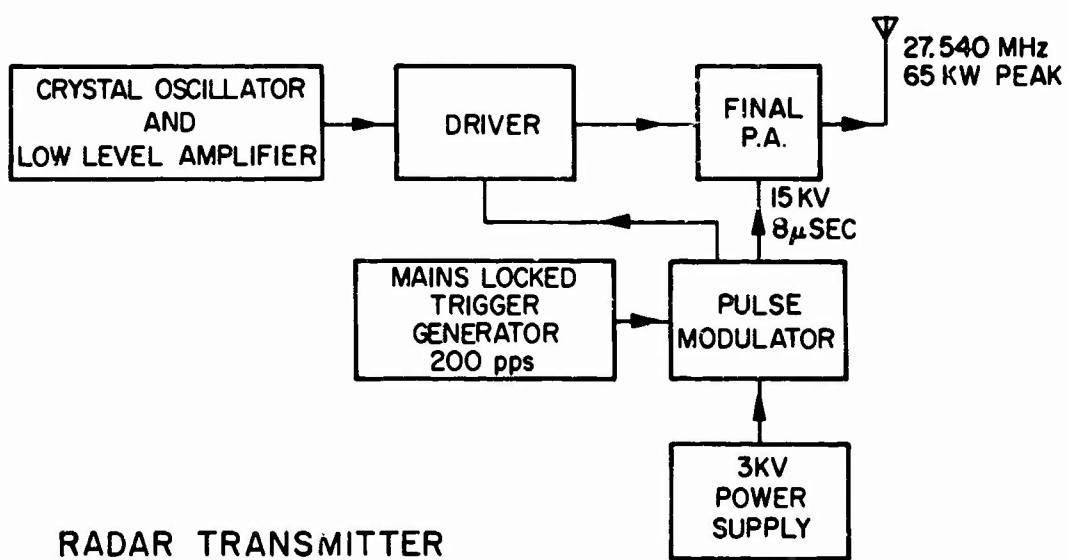
CW TRANSMITTER

Figure 4. Block Diagrams of the CW and Radar Transmitters

Figure 5. Block Diagrams of (a) the Equipment at the Main Receiving Station, (b) the Outstation Equipment

As explained above, the output of the cw transmitter is phase modulated periodically so that the phase is retarded linearly by 90° in 80 μsec and then restored linearly to zero in 20 msec. This modulation is achieved by phase modulating the signal from the low frequency crystal oscillator with a saw-tooth waveform to give a maximum deviation of 2 1/2°, and then frequency multiplying by 36 to give a 90° deviation at 26.77 MHz. The cw transmitter delivers 1,500 watts to the antenna.

The exciter of the pulse transmitter is crystal controlled and delivers a cw signal to the driver stage. The driver and final stages are pulse modulated with a line type pulser that generates 8 μsec pulses at a repetition rate of 200 sec^{-1}. The pulse transmitter delivers pulses with a peak power or 65 kw to the antenna.

3.2.2 MAIN RECEIVING STATION

A block diagram of the equipment at the main receiving station is given in Figure 5(a). Signals from the three principal direction finding antennas (1, 2, 3

in Figure 3) are passed via a system of asymmetrical time sharing, into two IF channels; to facilitate phase comparison the signal from antenna 2 is common to the two IF channels. The supplementary direction finding antennas 4 and 5 are connected to the two Doppler receivers which are also used for accurate measurements of the frequency and sign of the Doppler beat and the amplitude of the echo waveform. The bandwidths of the DF and Doppler receivers are about 7 kHz, which is sufficient to pass the "sense spikes" with relatively little distortion.

The ground pulse and radar receivers have bandwidths of approximately 150 kHz. The ground pulse receiver is connected to a horizontally beamed Yagi in order to provide a reliable trigger pulse for the time base of the radar display.

All receiving antennas other than the ground pulse antenna are half-wave dipoles à quarter wave above ground. The system accepts meteor echoes from all azimuths and from elevations within 60° of the zenith. The strength of the ground wave at the receiving station is determined by the geographical sighting of the system; the receiving sites are partially shielded from the transmitter by a low ridge near the transmitting site. Provision is also made for adjusting the height above ground of the transmitting antenna.

3.2.3 THE THREE OUTSTATIONS

These identical outstations each consists of a 26.773 MHz narrow-band receiver, similar to the Doppler receivers, and a 167 MHz IF transmitter. Since the ground wave provides a constant reference phase, no precautions are necessary to ensure phase stability. The receiving antenna is a $\lambda/2$ dipole, $\lambda/4$ above ground; the FM antenna is a Yagi beamed to the main station. The units are sunk in the ground to avoid vandalism and to obtain some measure of thermal stability; there is also provision for forced air cooling. A block diagram appears in Figure 5(b). Because of the low Doppler frequencies sometimes encountered, and to preserve ground wave information (this is necessary for the measurement of echo amplitude) the output of each receiver is chopped at a frequency of 2 kHz before it is passed to the link transmitters.

3.2.4 RECORDERS

Information is recorded on 35 mm film which is normally stationary but is set in motion for a short time, through a magnetic clutch, when a predetermined change in field strength в. one of the Doppler antennas at the main station indicates the probable presence o. a meteor echo. The recorder is divided into three sections, each with a vertical bank of cathode-ray tubes, clutch and camera.

The wind data is presented on three tubes, portraying respectively the outputs of the DF receivers, the Doppler receivers and the radar receiver. The turbulence data from the main station and the three outstations is presented on two

double beam tubes. Both these sections of the recorder are triggered by the echo itself, and the film reaches its full speed within 1/20 sec. Since most useful echoes persist for at least a few tenths of a second, there is no significant loss of information in this initial phase. In each case the film speed is 0.38 in./sec, adequate to resolve the maximum Doppler frequencies of approximately 30 Hz. The duration of the recording interval can be varied but it is usually set at about 1.3 seconds.

3.2.5 MAGNETIC TAPE MEMORY UNIT

The recording of the velocity and radiant data requires a more sophisticated approach. Firstly, much higher film speeds are needed to resolve the velocity diffraction patterns, whose frequency may be as large as 600 Hz. Secondly, the useful part of the pattern is generated before the meteor crosses the specular reflection point, and hence well before the normal trigger system is able to start the recording cycle. If triggered recording is to be retained, and this is highly desirable from the point of view of economy of film, some means is required of storing the velocity information until the trigger system is able to operate. This storage is performed in a multi-channel continuously moving magnetic tape memory unit, in which information is held for about 1.5 sec before being read off by the playback head, as indicated in Figure 5(a). Recording only takes place if the main trigger system has operated. The tape is modulated directly by the chopped signals received via the outstation FM links; in addition an output of one of the main station Doppler receivers is chopped before being applied to the tape. Information is displayed on three CR tubes and photographed on film moving at a speed of 1.9 in./sec.

In order to facilitate identification of the same echo on the three films, each echo is allotted a serial number which is printed on the films from three counters operated in coincidence. Each counter is illuminated by a short electronic flash at the end of each recording cycle. Time is recorded once every 15 minutes on the wind film and for every echo on the orbit film. A recording of a typical wind echo is reproduced in Figure 6.

4. DATA REDUCTION

The 35 mm films on which the data are recorded are projected in special viewers built for the purpose and the raw data, mostly times of maxima and min-ima of the recorded waveforms are read off in analog form, fed to an analog to digital converter and recorded on punched cards. Range, time, and echo number are read for each echo and manually punched on to the card.

The wind, turbulence, and orbit film records are read separately and all the data is processed on a computer. The computation of winds from the records is
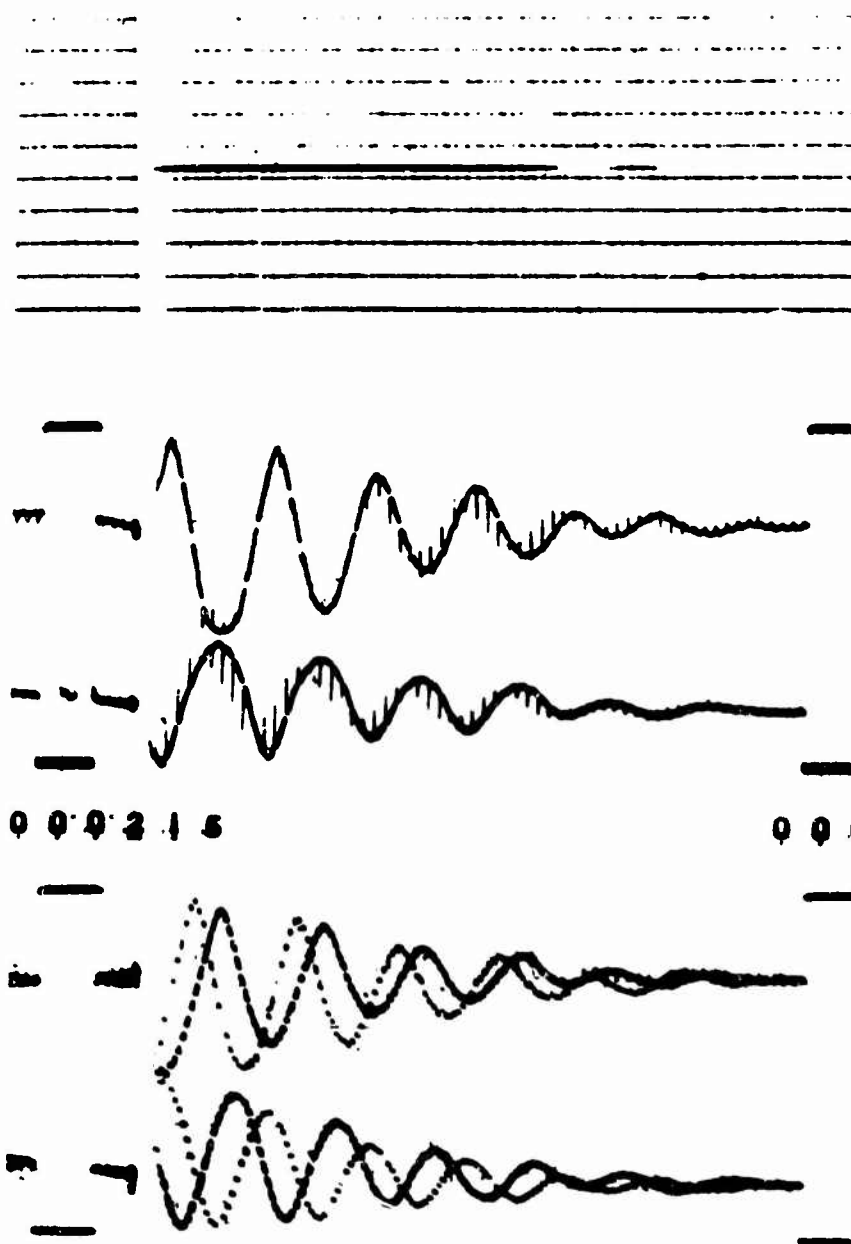
Figure 6. A Typical Echo From a Drifting Trail. The top trace gives the slant range, the center two traces the outputs of the two doppler receivers and the lowest four traces the outputs of the three DF receivers. The upper dark trace and the lower light refer to the same DF antenna, with the phase of one inverted

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carried out as follows: Punched cards containing the raw data are first checked on the computer for punching errors and the redundancy in the direction finding data is used to check the internal consistency of the data. Inconsistent records are noted and re-read. Any data that is still not internally consistent at this stage is excluded from further analysis. The total data rejected is less than 8%. At non-shower times the hourly rate of useful echoes varies between 5 and 50.

The diurnal, seasonal, and height variations of the wind are determined by setting up a model of the variation of the zonal, meridional, and vertical components as a function of time and height and determining the parameters of the model by comparison with the observed data using the method of least squares. The analysis is carried out on a computer according to the method developed by Groves (1959). Estimates of errors are made also.

Acknowledgments

Of the many people who have contributed to the development of this equipment the following have made major contributions: Dr. D.S. Robertson, who designed the major part of the prototype wind equipment; Dr. E.L. Murray and Dr. R.G. Roper, who designed much of the present equipment; Dr. J. S. Mainstone and Dr. C.S. Nilsson, who designed and built the prototype magnetic tape memory unit. The construction of both transmitters and the main receiving equipment was carried out by Mr. E.J. Welsby. The main financial support for this work has come from the Radio Research Board of Australia.

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Appendix

A Single Station Meteor-Wind Equipment

In 1957 a single station coherent radar system for measuring the drifts of meteor trails was installed at Mawson (68° S) in Antarctica. This equipment was based on the cw system in operation at Adelaide and incorporated the same method of determining the direction of the reflection point as has been described in Section 3.

The transmitter radiated a pair of 34 MHz pulses each 10 μsec in duration and spaced 130 μsec apart, at a pulse repetition rate of 750 sec^{-1}. A block diagram of the transmitter is shown in Figure A1. Phase coherence was maintained by feeding the pulsed stages from a crystal controlled cw exciter. The peak pulse power was 15 kW.

The receiving antenna array was similar to that shown in Figure 3 for the Adelaide system. A phase reference for the system was supplied by a low level 34 MHz cw signal from the exciter that fed a half-wave dipole antenna placed about 25 wavelengths from the receiving array and on a line through the axis of the array. The combination of cw reference phase and pulsed sky wave produced amplitude variations at the receiving antennas similar to that described in Section 3. Hence the resultant amplitudes at the receiving antennas contained all the information necessary for determining the line-of-sight velocity and the direction of the reflection point.

A block diagram of the receiving equipment is shown in Figure A2. Each echo consisted of a train of double pulses and in order to discriminate in favour of an echo the output of the radar receiver was fed to a 130 μsec magnetostrictive delay line and a coincidence detector. The five Doppler receivers were gated by a pulse from the coincidence unit in such a way that the receivers were only operative during the duration of the second pulse of each pair of echo pulses. The gated outputs of the Doppler receivers were peak rectified and passed through filters with a bandwidth of 50 Hz. The final Doppler waveforms had a signal-to-noise ratio equivalent to that of a cw system with a transmitter power equal to the mean power of the radar system and with a receiver bandwidth of 50 Hz.

The receiver outputs were displayed on oscilloscopes and the data recorded on 35 mm film in a similar manner to that described in Section 3.2.4 for the Adelaide equipment.
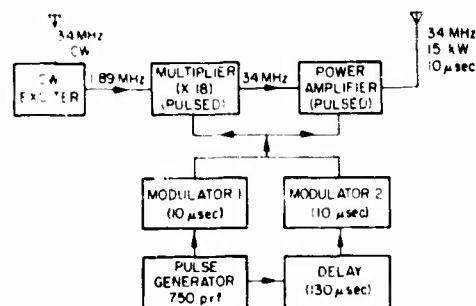
Figure A1. Block Diagram of the Phase
Coherent Double Pulse Radar Transmitter
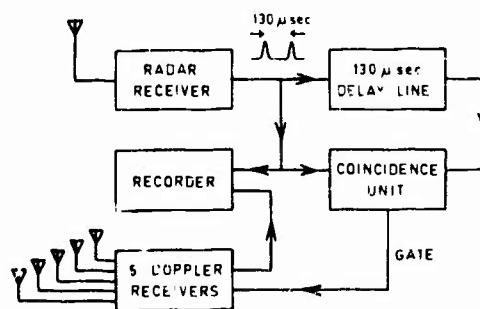Used at Mawson (68° S)

Figure A2. Block Diagram of the Pulse
Receiving Equipment and Double Pulse
Descriminator Used at Mawson (68° S)

DISCUSSION

MR. MYERS. Perhaps you have some questions for Dr. Elford?

DR. BARNES. It seems to me it the double pulse system would be a good way to eliminate pulse noise. This would be an ideal way of reducing some of the pulse radar noise we have at AFCRL.

DR. ELFORD. Right. This is true. You can make it more selective if you like; you can transmit four pulses if you so desire.

MR. MYERS. Perhaps you might like to comment on any undesirable feature you have found which you would like to re-do, now that you have obviously reached an improved level over several years ago? Is there any further direction in which you are looking?

DR. ELFORD. Well, we have reached the stage where we are faced with the data reduction problem, and our next question is, where do we go from here? Do we go into an analog digital conversion? If we do, this means large amounts of fundings.

THE FLOOR. I understood you to say that you ran four or five days a month; is that a 24-hour day?

DR. ELFORD. Twenty-four hours, yes. Normally, we run for five to seven days continuously. For special circumstances. we may run up to three weeks. We may leave two weeks of this for processing later. We try to keep one week's data out of each month read and processed within four weeks.

DR. GROSSI. How many components come back in your horizontal plane?

DR. ELFORD. Let me attack this in a slightly different way. The line-of-sight and drift components are measured from four stations. We then set up a model wind and we fit that data to this model using a least square analysis. We can put in as many components as we like, consistent with the observations that we have.

THE FLOOR. On any one meteor trail, do you measure only one line-of-sight?

DR. ELFORD. Yes, one line-of-sight. The spaced stations are so close together that all the components are essentially parallel.

THE FLOOR. There is only one component essentially, because your stations are not far apart?

DR. ELFORD. Yes, but we intend to put another station some 40 kilometers away.

MR. MYERS. This would be at right angles to the present transmitter - receiver line?

DR. ELFORD. Yes, at right angles.

MR. MYERS. Are there any other questions?

DR. BARNES. Although your line-of-sight is essentially the same, your specular reflection points are spaced so that the actual Doppler shift can be different from one point to another?

DR. ELFORD. Yes, this is essentially the data that Dr. Roper has used for studying turbulence.

MR. MYERS. I think this will be covered in our session Wednesday. It will be a major topic then.

THE FLOOR. What is the separation in height as far as reflection points on one particular meteor trail are concerned?

DR. ELFORD. With our present system this is two kilometers. We are now pushing this up to four kilometers and it will eventually be fourteen kilometers, if we can achieve that separation on one trail.

THE FLOOR. You just mentioned that you had separations to, let us say, up to two or four kilometers. This separation can be measured fairly accurately. Do you have similar accuracies in the absolute heights?

DR. ELFORD. Maybe I can refer that answer to Dr. Roper. He is going to go into that tomorrow.

DR. ROPER. In fact, we get an absolute resolution about plus or minus two kilometers, but the separation resolution between reflections on any one trail can be measured to the order of about 30 meters per kilometer of separation.

DR. ELFORD. One must never overlook that the reflection segments are on the order of a half a kilometer long, so you are measuring the separation of two segments to an accuracy of 30 meters, but the segment can be five hundred meters, or so, long.

MR. MYERS. Our next paper is by Dr. Grossi, who will explain the Havana system.

II. The Meteor Radar Network
at Havana, Illinois

Dr. Maria D. Grassi
Smithsonian Astrophysical Observatory
Cambridge, Mass.

The system that I will describe is installed in Havana, Illinois, at the Ionospheric Station of the National Bureau of Standards. The network is a rework of a six-station non-coherent radar system that has been operated since 1961 by the Harvard Radio Meteor Project.

We have now a new eight-station multistatic phase-coherent network (Figures 1 and 2) from which we expect to collect coherent meteor echoes from a volume in the upper atmosphere, roughly 30 by 50 kilometers horizontally, and 16 kilometers vertically, with the vertical dimension extending from 80 to 96 kilometers, and with approximately 240 radar-resolvable cells. The volume under radar-patrol is approximately above Decatur, Illinois.

When a cell is occupied by a meteor trail, echoes are received at the main site (where the VHF transmitter is located) when the trail in the cell is tangent to a sphere centered at the main site and with radius equal to the slant distance of the cell from this site (backscattering case). Site No. 7 (Figure 1) receives an echo when the trail is tangent to the prolate spheroid, having as foci sites 7 and 3.

The same holds for the outlying sites, Nos. 1, 2, 4, 5, and 6; these sites, however, are so near to the main site that each related prolate spheroid can be
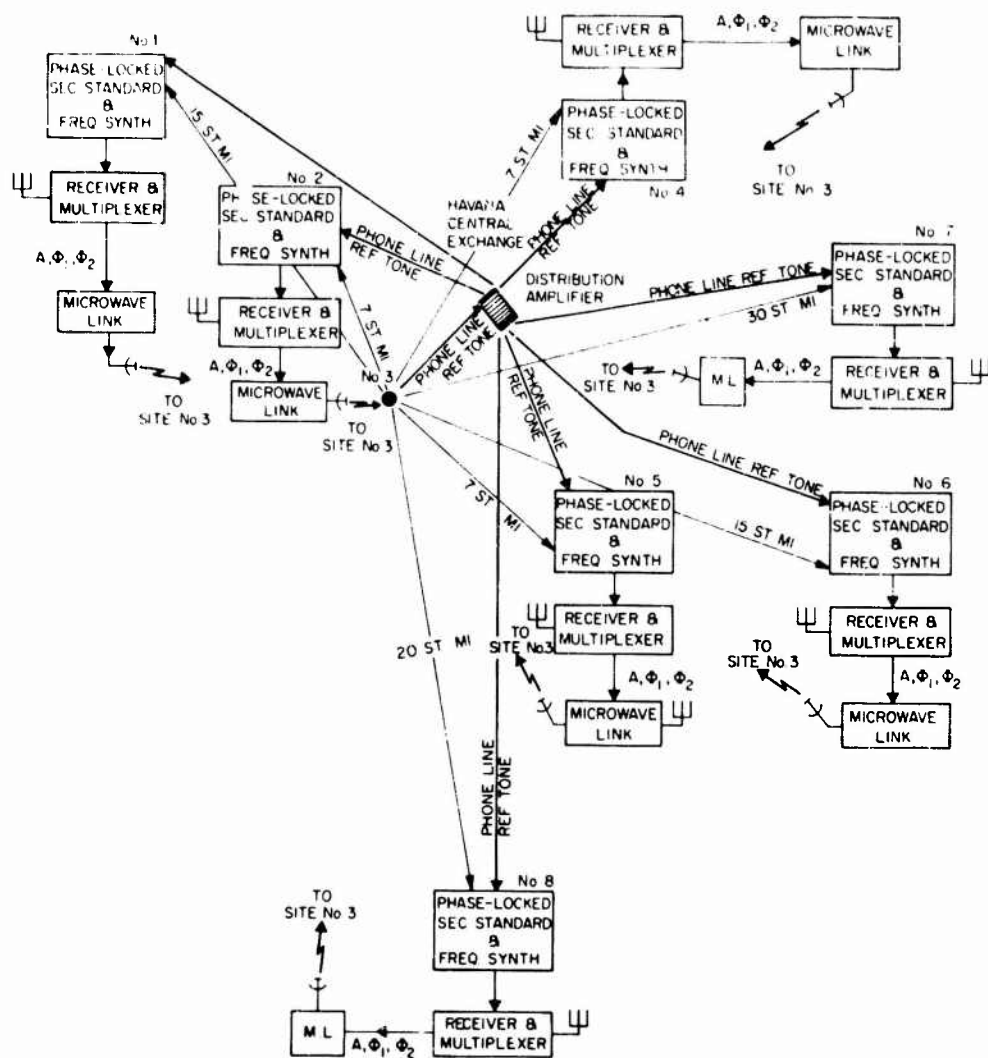
Figure 1. General System Layout
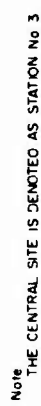
Figure 2. Main Site Simplified Block Diagram

approximated by a sphere centered at a ground-based point midway between each location and site No. 3.

At the main site (where a 3-megawatt peak power 40.92 MHz transmitter is located), a dual-channel receiver provides range, angle of arrival, Fresnel pattern, and Doppler information (radial velocity) on the echoes backscattered by a trail.

Figure 3 depicts the double-trough antenna of the main site. The two sections, in an interferometric arrangement, provide the echo's angle of arrival.

At each one of the remote sites, No. 7 and 8, a single-channel receiver provides range, Fresnel pattern, and Doppler information (wind velocity along the bisector of the angle which separates the site from the transmitter location, as seen from the trail) on the echoes forward-scattered to the site from the trail.
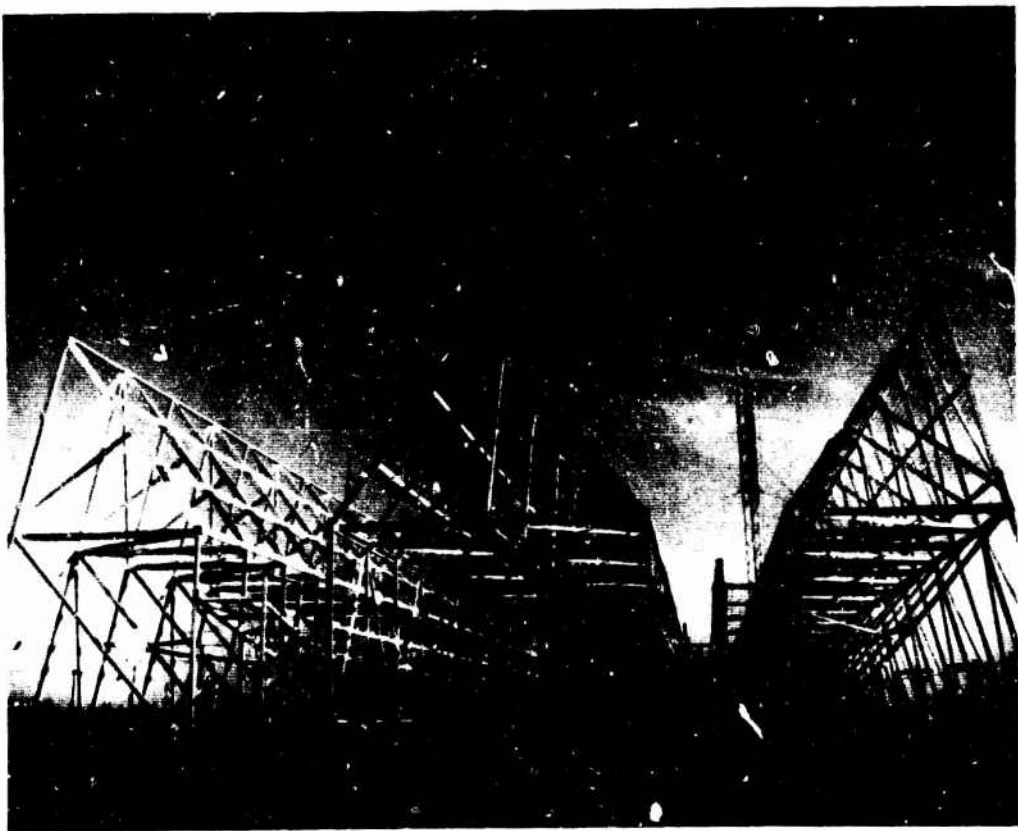
Figure 3. The Double-Trough Antenna at the Main Site (site No. 3)

Identical equipment is available at each one of the outlying sites, which are grouped around the main site and complement it for radial wind velocity measurements.

A three-dimensional wind determination in each cell requires that echoes from the cell at a given instant be received at the main site (or, which is equivalent, at one of the outlying sites) and at both remote sites. Because, in general, a single trail does not scatter echoes in all the needed directions, we have to wait until the cell is crossed by more than one trail nearly at the same time, in order to obtain the basic echoes we require for the wind measurement.

Assuming that 15 samples per hour for each cell are sufficient to describe adequately the wind circular period, we have that $240 \times 15 = 3,600$ independent samples an hour are required in the overall volume surveyed.

We can realistically expect to collect this amount of information per hour with the new eight station radar network. The projected frequency of reducible meteors is in fact 500 an hour, with 2,000 one-dimensional wind velocity determinations per hour, and 4,000 independent samples an hour (each one-dimensional wind velocity determination gives two pieces of information because it provides also the wind derivative with respect to position along the trail).

From the one-dimensional wind velocity determinations (2,000 per hour), from the small amount of two- and three-dimensional data (100 an hour and 200 per hour, respectively) and from consideration of continuity, quasi-real-time wind pattern reconstructions can be performed.

This is mechanized by conveying to the main site for processing in a digitizer and recording in a multichannel digital tape recorder all the data collected by the eight stations, and by playing back the tape in an IBM-7090 computer, appropriately programmed to printout and/or plot the three-dimensional wind profiles.

Expected system accuracies are ± 15 m in range and height; ± 1 m/sec and ± 3 m/sec in radial velocity for trail altitudes below and above 95 kilometers, respectively, ± 4 m/sec and ± 12 m/sec in velocity components perpendicular to the radial ones, again for altitudes below and above 95 kilometers, respectively.

A system calibration procedure and the related calibration equipment are under development with the aim of reaching an overall radar network accuracy of better than 40% in echoing cross-section measurements. Transmitter power, antenna gain, and receiver sensitivity must be known within 0.8 dB, each, for the overall accuracy goal to be met.

During August and September 1965, the network was installed in the field, and preliminary one-dimensional wind information collected from sites 3, 7, and 8. Figures 4 and 5 show samples of records obtained at site 3.

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ϕ_2 leads ϕ_1 therefore meteor trail approaches site #3

Distance from site #3	142 kilometers
Doppler frequency	7.4 cycles/second
Wind radial velocity	27.5 meters/second
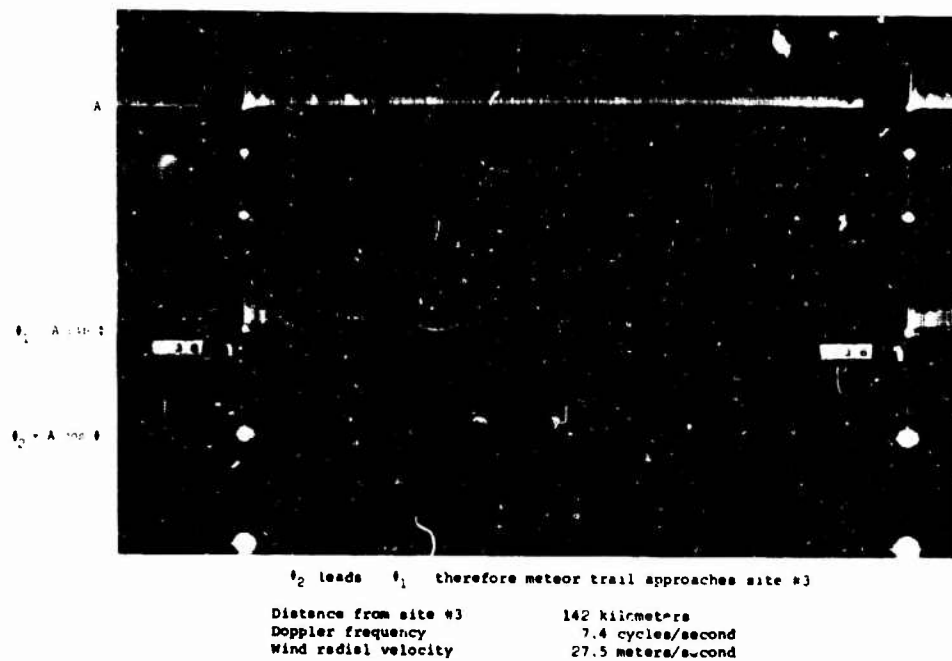
Figure 4. Sample of One-dimensional Wind Measurement (meteor trail approaching site No. 3)

ϕ_1 leads ϕ_2 , therefore meteor trail recedes from site #3

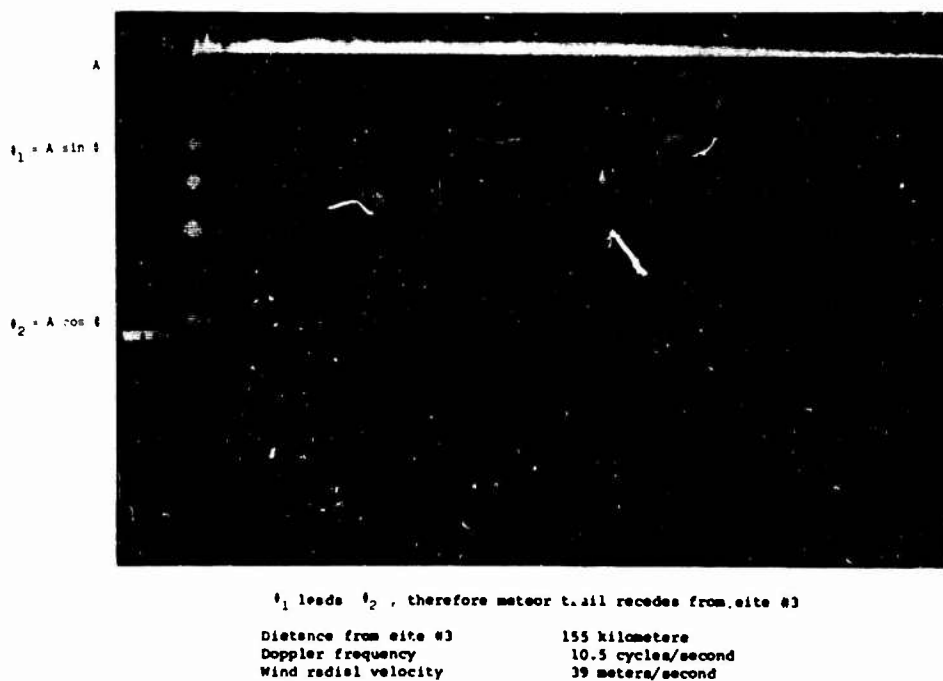| Distance from site #3 | 155 kilometers |
| Doppler frequency | 10.5 cycles/second |
| Wind radial velocity | 39 meters/second |

Figure 5. Sample of One-dimensional Wind Measurement (meteor trail receding from site No. 3)

DR. BARNES. You mentioned a tape recorder. What type of a recorder is that?

DR. GROSSI. A Potter unit. The overall subsystem was made by Baird Atomic, and it contains a digitizer, a tape recorder, a multiplexer plus associated logics units.

DR. BARNES. Is that a step recorder?

DR. GROSSI. Yes, every time the tape recorder is switched "on", it goes ahead for one tenth of a second to one second.

THE FLOOR. Do both maxima and minima of the meteor flux meet your echo rates requirement?

DR. GROSSI. Yes, it is hoped that Dr. Southworth will have at least 500 reducible echoes per hour.

DR. SOUTHWORTH. That is what we want.

DR. GROSSI. We have less than that now, something like 100 meteors per hour.

DR. SOUTHWORTH. It varies.

DR. GROSSI. On the average we have now approximately 100 meteors per hour. We are therefore a factor of 5 below the nominal requirement of 2,000 one-dimensional velocity samples per hour. In effect we are in a slightly better shape because 3,600 and not 4,000 are the needed independent samples per hour, which bring down to 450 the number of meteors to be observed in one hour.

THE FLOOR. This expectation is obviously due to your very high transmitter power. You could compare that with the rate of about 250 meteors per hour observed with other existing systems.

DR. GROSSI. Yes, the rate you mentioned is obtained with radar systems having radiated power levels in the order of 20 kw peak.

THE FLOOR. You expect a fairly high return of data; you said something about 3,600?

DR. GROSSI. Yes, we need 3,600 samples per hour.

THE FLOOR. Don't you expect some problems with overlapping echoes? I don't think the samples are really individual meteors.

DR. GROSSI. Right, the individual meteors are only 450 to 500.

MR. MYERS. How many meteors will the tape record when it moves one step?

DR. GROSSI. Only one each time. We have to remember that a meteor usually appears in more than just one channel of the tape recorder, so you may have four or five pulse trains on the multichannel tape for every meteor trail.

MR. MYERS. But what happens if more than one trail provides echoes while the tape recorder is running for another trail?

DR. GROSSI. We lose the trails arriving after the first. The system locks on the first trail received and rejects the remaining ones.

THE FLOOR. When you say you hope to get 500 reducible meteors per hour, is that the actual meteors, so you will have eight samples on each meteor?

DR. GROSSI. Yes, four times two is eight.

THE FLOOR. Do you remember what your average power is?

DR. GROSSI. We can compute it quickly. It is about 13 kW.

THE FLOOR. I don't know if there is a convenience in going very high (in power).

DR. GROSSI. Yes, there is. You can increase the number of reducible meteors again. The higher you go, the more meteors you are able to reduce, until you satisfy your rate requirements.

THE FLOOR. Have you any idea how many unusable echoes you get in periods of high average rate?

DR. GROSSI. We get a total number of meteors at least ten times larger than the reducible ones.

DR. SOUTHWORTH. We are counting echoes now with the rate running eight to ten thousand per hour.

DR. GROSSI. So actually I should have said 80 or 100 times.

THE FLOOR. You mentioned that you hoped to get 500 reducible meteors, but now you have only between 100 and 200. What are you doing to get your 500?

DR. GROSSI. We think it is a matter of adjusting thresholds in the pattern recognition units and of reducing the time intervals during which the system is inhibited by the logics to lock on arriving pulse trains. We don't believe that the receivers systems have poor sensitivity. We believe this is a matter of system adjustment.

THE FLOOR. The use of the Fresnel pattern for trail positioning introduces a lot of problems in data reduction. Couldn't you use a different and simpler approach by determining the position of the trails by measurement of angles of arrival?

DR. GROSSI. Would you still look at the same volume in space?

THE FLOOR. Yes, I am suggesting that you have this small volume in space looked at from a large number of areas some place on the ground. Had you thought of possibly using some sort of phase method to get direction of arrival of the signal?

DR. GROSSI. You know that we measure this quantity at site 3.

DR. SOUTHWORTH. We can reduce the number of stations if you want to measure, say the phase of arrival. It has been our experience, however, that it is very hard to keep simple antennas stable in the wind. You can rely on them to within 1° or 2° of angle of arrival.

DR. GROSSI. This morning Mr. Myers was talking about cost. I don't have firm numbers about what our system would cost, after the developmental work we performed, but I guess it is between a quarter of a million and a half million dollars.

PROF. PETERSON. I think it is a question of really what do you want to get out of it. If you want the average wind or wind shears as a function of height, you probably don't need the three-dimensional data, and probably you don't need density measurements either, but if you really want it in three dimensions, then you have to have a system like this.

DR. GROSSI. Yes, that is exactly how we feel on this issue.

MR. MYERS. Our next speaker is Mr. William Ramsey. He will give us a paper on the AFCRL transportable meteor trail system.

III. The AFCRL Pulse Doppler Radar for the Determination of Winds and Density from Meteor Trails

William H. Ramsey
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Robert F. Myers
Air Force Combridge Research Laboratories
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Abstract

The design of a 40-kW, 36.8/73.6 MHz pulse Doppler radar for the determination of winds and density from meteor trails in the 80 to 105 km region is presented. The set utilizes a 40 μsec pulse at a pulse repetition frequency of approximately 500 pulses per second.

The meteor trail information may be displayed on oscilloscopes and photographed for manual interpretation. Additionally, the data are processed by a small computer on a pulse-by-pulse basis for each trail and the derived information stored on magnetic tape for further data processing.

1. INTRODUCTION

In order to develop forecasting techniques applicable above 32 km, a source of density and wind data was desired for the 80 to 105 km interval which would

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permit widespread geographical coverage with a reasonable network operating cost.
Although network soundings of winds and temperature as a function of height employ-
ing small solid fuel rockets are routinely made, the current vehicles do not reach
the 80 to 105 km portion of the region of interest. Larger and much more expensive
vehicles are available, but they are not being flown in a regularly scheduled net-
work, and the cost per sounding is quite high. Satellite data can provide densities
down to about 200 km or possibly 160 km, but the region above the rocket network
is not covered routinely at the present time.

A survey was made in 1962 of the reported techniques which had been used to
obtain winds and density from the radar measurements of ionized meteor trails.
If the data proved to be valid, the continuing supply of free sensors appeared to be
quite attractive from a systems viewpoint. The ground equipment appeared to be
no more expensive or extensive than the radar associated with a rocket network for
accurate wind data and the flight equipment would be literally nothing compared to
the payload of instrumentation for pressure, density or temperature.

Of the two parameters, density and winds, density was believed to be the more
important and the more difficult to obtain. The method employed by Greenhow and
Neufeld (1960) to determine the density as a function of the rate of decay of the
signal strength from underdense meteor trail held promise if some of the basic
assumptions on which it was based could be verified. The continuous-wave tech-
niques requiring multiple installations at each site would have more difficult data
processing requirements if a network of stations were collecting routine data, than
would a single station utilizing the Greenhow technique. For these reasons, it was
decided to try to repeat the Greenhow (1957) experiment as given in the IGY instruc-
tions and see if data could be gathered using the same measurement scheme with
the added goal of reducing the data in real time to manageable numbers which could
be computer-processed. As Greenhow stated, the real advantage of his method was
the ability to average out the effects of large scale turbulence by making a consider-
able number of wind determinations spread throughout a region equal to or larger
than the eddy size.

It was thought that there was some possibility that a large multistation installa-
tion with an extensive antenna system could provide somewhat higher accuracy in
the individual determination of height for a single trail. This possible advantage
would be offset by the smaller number of trails seen simultaneously by the multiple
receiver sites and by the more complex data handling required for multistation data.
If measurements from a dozen geographical sites were required for development of
a forecasting capability, then the mere size of the sites and the cost and complexity
of data acquisition and processing in a multistation technique made prior exploration
of the single station techniques highly desirable. The character of the data sought
was somewhat different from that of normal meteorological measurements. Here

the experimenter had no control over the scheduling of the original data in the sense that the trails could only be accepted or rejected and not commanded to appear at some regular interval.

The meteorological characteristics of the 80-105 km region are believed to involve diurnal changes, tidal changes, and seasonal changes as well as perturbations on scales between these daily, quarterly or semi-annual cycles. The eddy sizes range from small ones of the order of meters to very large ones of many kilometers, if one considers the scale of diffusion and synoptic features, and to global dimensions if the seasonal changes are considered. Obviously, if one wanted to study the diffusion coefficients at these altitudes, many individual observations would have to be compared. If one limited the use of the data to diurnal or longer time scales, the individual observations could be smoothed considerably. Individual observations would not have to be used after the limits of internal accuracy were established during the initial evaluation of the technique, but only means and standard deviations of groups of measurements would be required to investigate the time scales of usual meteorological concern.

The published work on the meteor trail region supported these assumptions and indicated that sufficient numbers of trails could be obtained to permit averaging over hourly intervals to form the common meteorological data base.

2. DATA-PROCESSING CONSIDERATIONS

Any experiment designed to test the feasibility of a technique for application to an operational problem or a network operation must consider the data-processing as a primary element in the design and planning stage, and not as an after thought to be attached after the data acquisition has taken place. Analog, digital, and hybrid techniques of processing were considered for this application. Since there was a need for developing an intimate feel for the data characteristics, there was an obvious need for a visual display and record for educational purposes as well as monitoring of the output of the system. A display and photographic recording of scopes would provide this initial data. It would be very burdensome to operate routinely with large quantities of manually processed data from a network of stations. The annual data processing cost might well be expected to be several times the equipment cost. Further refinements could be made in the degree of analog processing before the recordings were made, and the end product might require less editing, and less manual manipulation; but, in general, there would always be a place where people would have to read off the data from film or pictures to get quantitative numbers for summarizing.

The planned system would initially take oscilloscope photographs to provide data to develop the criteria for recognition of a trail and for working out the processing system which could be implemented digitally in the longer term network operation. The successful implementation of the WIND (Weather Information Network Display) system at Cape Kennedy provided a base for the confidence that an automatic digital system could be economically implemented for the meteor trail problem.

In the WIND system (Myers, 1963) data were acquired from a network of many kinds of meteorological sensors, and a real-time map of the wind flow patterns was obtained with a prediction of the quantitative diffusion of a cloud. A small general-purpose computer controlled the data acquisition, made the calculations, fed the displays, and provided a data record for research and climatological purposes. In the Meteor Trail Radar the same philosophy could be applied successfully if there were an adequate number of trails observed per hour, so that statistically meaningful data rates could be obtained when a portion of the data was ignored or rejected, and if the signal characteristics permitted completely objective logical decisions to be made about the signal returns. Consideration of the problems expected indicated that the feasibility could be demonstrated in a positive or negative fashion with a computer of the same general type as that used in the WIND system, although it was recognized that it would not be an optimum machine for the application.

3. SYSTEM CRITICAL AREAS

During the basic planning of the system, a number of critical areas were recognized which must be handled carefully or the data would not be of adequate quality to permit application to the problem of prediction of the wind and density variations in the upper atmosphere.

The problem of space location of the data was considered from several viewpoints: height accuracy required to be meaningful meteorologically, consideration of the azimuth characteristics, the required accuracy in drift velocity of the trail, and the effect of antenna beam width. In order to be useful meteorologically, the height assigned to any trail should be accurate to within 1 kilometer, although the data would be of some use if the height accuracy were ± 2 kilometers. In an ideal system every detectable meteor trail within range would be useable. This would be the case only if the azimuth were known as well as the elevation angle. A secondary characteristic would be the larger area from which the observations were obtained as compared with the area sampled by a fixed antenna positioned along a coordinate axis. The constraint imposed by the Doppler measurement providing a

a dial velocity component implies that, to be useful, the vertical component at these altitudes should be small with respect to the horizontal compone   The maximum wind velocity unambiguously determined by the system is rela ed to the carrier frequency and the pulse repetition rate. The minimum usable pulse repetition rate is twice the maximum Doppler frequency expected, which in turn sets a bound on the design characteristics of the pulse Doppler radar.

A major problem area was expected to be the influence of radio frequency interference on the performance of the system. It was felt that if the system could operate only at exceptionally quiet locations, the constraints placed on station location in forming a network would be too restrictive. The possibility existed that the influence of noise on returns could be minimized by such an abundance of returns that some small percentage of valid trails would be obtained which would be sufficient statistically to provide adequate meteorological data. The real kernel of the problem was to perform an experiment which would permit an assessment of the power of combining components of wind, sorted according to height intervals, in overcoming the many known difficulties in recording the specular returns from meteor trails. The convenience of operating the set near AFCRL to permit maximum education of the experimenters was also a factor in trying to operate in a noisy environment. A period of observation on the two frequencies, 36.8 MHz and 73.6 MHz was carried out before the final site selection was made, and the pertinent parts of the radio frequency spectrum did not appear unduly noisy. The ignition noise of cars entering and leaving the parking lot was felt at that time to be the worst feature observed.

Subsequent to these tests and prior to delivery of the Doppler radar, an unfortunate addition was made to the scene by AT & T. A Bell Boy installation, which was put on the air in the Boston area, flooded the countryside with 10 '-microvolt signals at 35.66 MHz, about a megacycle away from the 36.8 MHz frequency assigned for the meteor trail radar. This was discovered when the radar was installed and connected to the antennas; receivers were saturated with the almost continual signals from the Bell Boy paging system. Narrow crystal filters were immediately ordered for all the receivers and this source of annoyance was eliminated after considerable delay in getting the system on the air and checked out.

The meteor trail system was to serve as a data acquisition device with a digital processor on-line, and, as a result, it was necessary to design the system timing from the beginning to permit operation of the transmitter and processor under computer-control after implementation of the data-processing subsystem. In particular, the small general-purpose computer (a Packard-Bell PB 250) available for feasibility tests was not capable of random access to its memory since it used nickel delay lines for its serial memory, and a lack of flexibility in setting

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the radar system characteristics could easily have prevented use of this particular machine to determine the feasibility of the technique.

A particularly difficult area, which was recognized from the beginning, was the establishment of adequate selection rules for determining when a signal was a meteor trail, when it was an underdense trail, when enough of the trail had been received to give good data for computation purposes, and how to minimize the effect of noise. The only practical plan was to place the station into operation and acquire enough film of the actual signal appearing on the display oscilloscopes to analyze the signal characteristics and the interference characteristics to define the selection rules applicable to the system as it existed.

4. SYSTEM DESIGN PARAMETERS

The design goals for the meteor trail radar set were stated as follows:

The basic set shall be a coherent-pulse Doppler radar operating on a fixed frequency of either 36.8 MHz or 73.6 MHz with fixed antennas aligned to the North and to the West.

The output of the system was to give:

elevation angle of the incoming signal,

slant range of the specular reflection,

alternate samples of the north and west wind components over a velocity range of 10 to 200 meters/second, and

normalized amplitude of signal returns.

The characteristics of the radar set were desired to be:

100 kW peak power;

pulse lengths of 5, 10, 50, 100, 200, and 400 μsec (selectable);

pulse repetition rates of 500, 200, 100, and 50 pps (selectable);

maximum duty cycle of 2%;

matching of the coherent receivers used in determining the elevation angle to not less than 2% in gain and bandwidth;

adjustable bandwidth in the receivers for best signal-to-noise ratio for the selected pulse; and

photographic oscilloscopic recording of:

the amplitude ratio of the signals from the elevation antennas,

the time-amplitude of the meteor return,

the coherent Doppler video,

the slant range,

the time of the return,

the set of antennas in use.

5. HARDWARE

The radar in the AFCRL Transportable System is a coherent master oscillator-power amplifier configuration. Briefly, the antenna subsystem is based on ruggedized Yagi-Uda radiators; the duplexer is a classic gas tube branched configuration; the transmitter utilizes tetrode amplifiers in the intermediate and final power amplifier stages. The receiving subsystem consists of three superheterodyne channels employing logarithmic or linear post amplifiers, and the data-processing subsystem is solid state and is designed to prepare the received signals for display on cathode-ray oscilloscopes or for entry into the input buffer of a small general-purpose digital computer which performs on-line processing.

5.1 Antenna Subsystem

The basic radiator used is a five-element ruggedized Yagi-Uda antenna obtained from a commercial vendor. For each frequency, four of these radiators are arranged in a two by two array atop a 50-foot tower with the bore sight axis inclined at 45° to the horizontal as shown in Figure 1.

These antenna assemblies are arranged to point approximately north and approximately west. They are used to propagate pulsed energy at either 36.8 MHz or 73.6 MHz as well as to receive reflected energy from the ionized trails. From this energy the receiving sub-system extracts Doppler, decay, and range data.

The two-by-two arrays have the following characteristics:

frequency: 36.8 and 73.6 MHz;
polarization: horizontal;
elevation beamwidth (3dB): 50°;
azimuth beamwidth (3dB): 30°;
gain (above isotropic): 14 dB;
input impedance: 50 ohms.

The coaxial cables from each of the arrays are run to the mobile enclosure where a coaxial switch can be operated to switch from north to west and back at either of the two frequencies of operation. RF interconnections of the arrays are shown in Figure 2. The condition of the coaxial lines and antennas is monitored by use of a new technique known as time domain reflectometry (TDR). Some typical oscilloscope displays of TDR are shown in Figure 3.

The Time Domain Reflectometer technique locates and determines the magnitude and nature of discontinuities in high-frequency transmission systems. A voltage step with a fast rise is applied to the transmission line. A reflection occurs each time the step encounters an impedance mismatch. This reflection is added to the incident wave and is displayed on the CRT of the TDR. The time
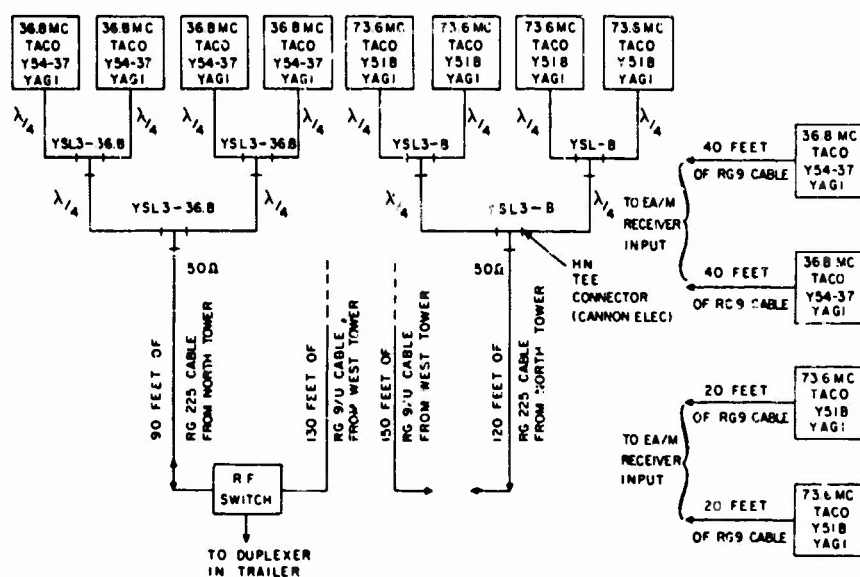
Figure 1.

Figure 2. RF Interconnection of MTR Antennas
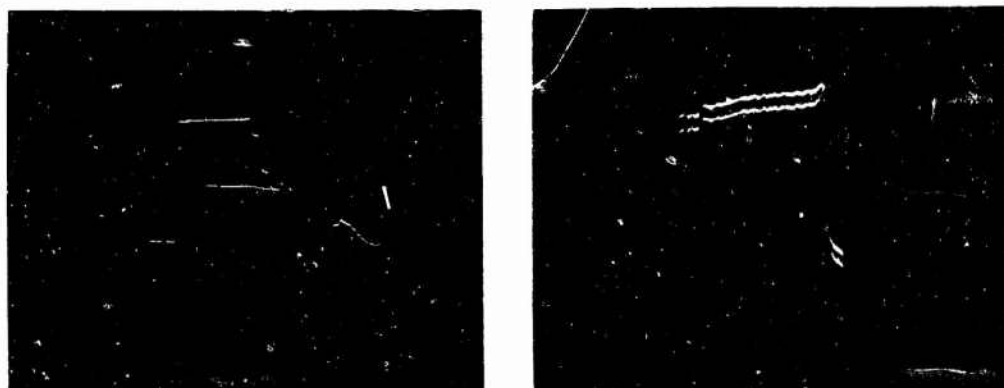
NORTH ARRAY, 100 nsec/cm, 0.2ρ/cm   NORTH ARRAY, 200 nsec/cm, 0.02ρ/cm
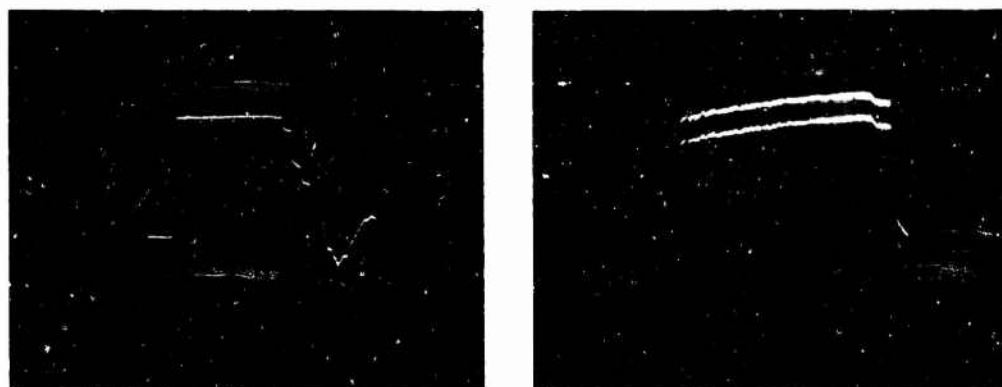
WEST ARRAY, 100 nsec/cm, 0.2ρ/cm   WEST ARRAY, 200 nsec/cm, 0.02ρ/cm

Figure 3.

required for the reflection to return to the sampler in the TDR locates the distance to the discontinuity. The shape and magnitude of the reflected wave indicate the nature and value of the mismatch which can be resistive, inductive, or capacitive.

Installed in one of the equipment racks is a unit that we call the antenna programmer. It is a chassis consisting of relays and timers which control the actuation of the two rotators and the coaxial switch in such a way that transmission and reception of rf energy is directed in one of two orthogonal positions for a preset time interval ranging from one minute to one hour. The programmer also has a manual mode where the individual antennas may be rotated or switched.

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A separate antenna assembly is used to obtain the elevation angle of the re-
flected energy. Two Yagis are placed one-half and one-quarter of a wavelength
above a reflective surface. The latter is a 50-by-100 foot framework which is
raised to a height of 14 feet. The spacing of the copperweld wire mesh forms one
foot squares. The interference patterns generated by each of the radiators with
the ground plane is shown in Figure 4.

By comparing the amplitudes of the reflected signal at the output of each of
these antennas, it can be seen that the elevation angle is determined uniquely but
with a modest accuracy. These antennas are mounted on piping which is supported
by rotators similar to heavy duty amateur radio units. The rotators are turned
via a program in synchronism with the rf coaxial switch connecting the two tower
arrays.

5.2 Duplexer

To permit transmission and reception on the two-by-two array, it is necessary
to use a gas tube duplexer to perform the rapid switching between transmit and re-
ceive conditions. This unit is a conventional branch duplexer as shown in Figure 5.
The unit is designed to be operated at either 36.8 MHz or 73.6 MHz by simply chang-
ing transmission line lengths.

During transmission the left short transforms to a low impedance at the left
junction. When the tubes fire, a high impedance is formed at the left junction.
This combination transforms to an open at the antenna port. Similarly, the trans-
mitter port is an open resulting from the right junction, resulting in a low imped-
ance path from the transmitter to the antenna ports.

During reception the shorted stub and open stub at the left and right junctions
form high impedances. Reflected energy will enter the duplexer and go left be-
cause the upper short reflects an open at the antenna port.

Because the isolation between the transmitter and receiver is only 20 to 30
decibels, a one milliwatt solid state limiter is used after the receiver port to pro-
tect the receiver front end from transmitter leakage. The limiter has two stages
using PIN and varactor diodes.

5.3 Transmitter Subsystem

The transmitter is a master oscillator-power amplifier configuration designed
to amplify the output of a stable crystal oscillator to a level of 40-kW peak power
with a maximum average power level of 800 watts at either of the two frequencies,
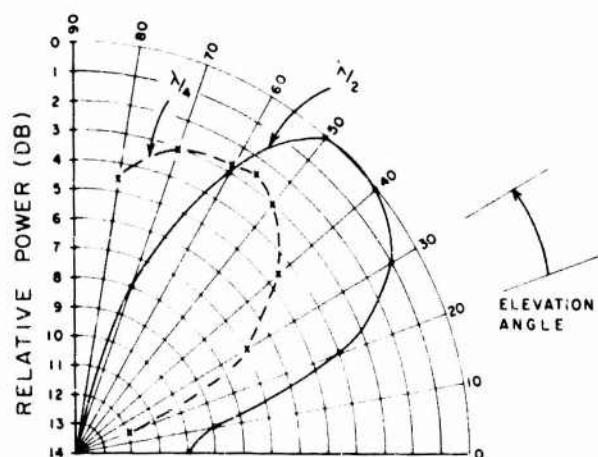36.8 or 73.6 MHz. A block diagram of the low frequency is shown in Figure 6.
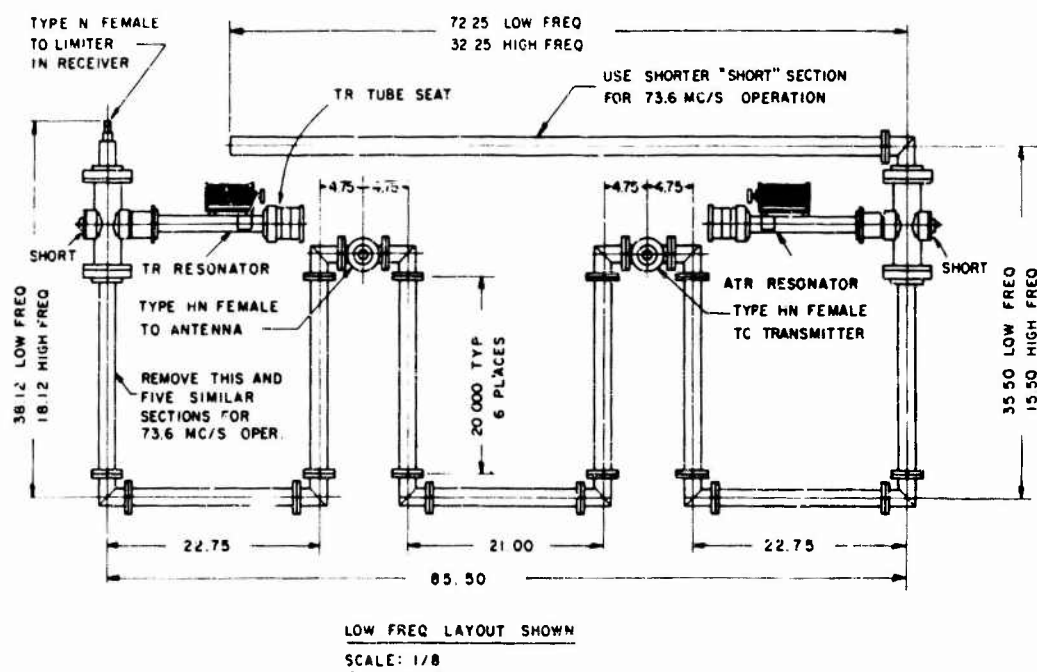
Figure 4. Patterns of EA/M Antennas

LOW FREQ LAYOUT SHOWN
SCALE: 1/8

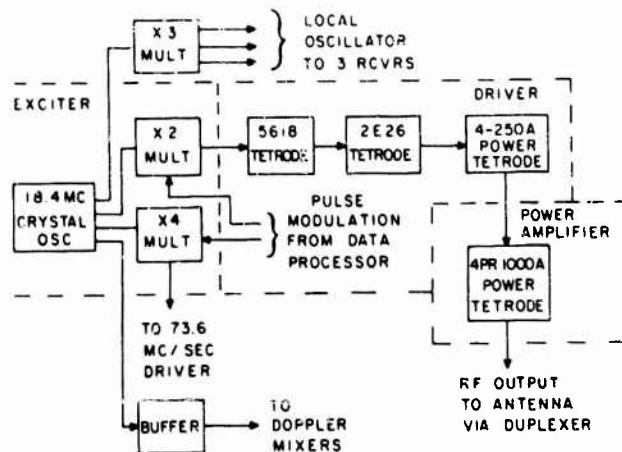Figure 5. Duplexer Layout, Type MPD-24
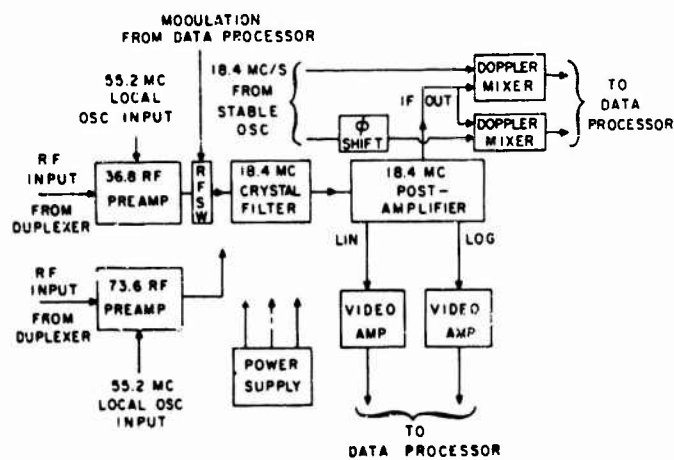
Figure 6. 36.8 MHz Transmitter

Figure 7. Receiving Subsystem

A 73. 6 MHz transmitter using a similar tube line-up is provided so that the opera-
tion frequency of the radar may be changed with a minimum of adjustments.

5.4 Receiving Subsystem

The receiving subsystem consists of three identical superheterodyne receiving
channels. Each channel has two radio frequency preamplifiers as shown in Figure 7.
The preamplifier gains are about 30dB. The 55.2 MHz local oscillator frequency
was chosen to allow generation of the same intermediate frequency for either the
36.8 MHz or 73.6 MHz operating frequency. Crystal filters were introduced after
the initial design was completed to allow narrowing of the system bandwidth from
several MHz to 300 kHz, the maximum required for the pulse lengths used. Adja-
cent channel interference was severe before the filters were installed. A fast-
acting radio frequency switch was introduced before the filter to prevent ringing
of the filter and overloading of the receiver by the main bang of the transmitter.

The output of the crystal filter is fed to a post amplifier which can operate in
either the linear or the logarithmic mode. The receiver dynamic range in the
logarithmic mode is about 65 dB, while the receiver range in the linear mode is
greater than 85 dB. Some of the pertinent receiver characteristics are shown in
Figure 8.

The intermediate frequency output before detection is fed to the Doppler mixers
whose other outputs are the second local oscillator signal at 18.4 MHz. Notice the
90° phase shift in the one mixer output. Simple trigonometric manipulation will
show that one can determine whether the wind is toward or away from the radar by
observing the phase relationship at the Doppler mixer outputs. The outputs from
the Doppler mixers are sent to the data-processor.

The receiving channel known as the DDR (Doppler, Decay, and Range) ampli-
fies the signal return in the logarithmic mode to make the determination of the
rate of signal decay simpler in the data-processor.

The receiving channels known as Elevation 1 and 2, which amplify the signal
returns giving amplitude data for the elevation angle measurements, are operated
in the linear mode when the data is photographed for manual processing and in the
logarithmic mode when the data is routed to the computer for automatic processing.
Video amplification is added to the detected signals between the receiver output and
the data-processor.

Figures 9 and 10 show exterior and interior views of one of the receivers.

5.5 Data Processor Subsystem

The data processor shown in Figure 11 is a completely solid state unit designed
to convert the various outputs from the receiving channels into signals appropriate

MODEL NO. R100S23
RECEIVER NO. ___3___
SERIAL NO. __1-.24-3__

	Channel A	Channel B
RF Center Frequency	73.6 MHz	36.8 MHz
Bandwidth (3dB linear mode)	1.6 MHz	1.5 MHz
*Noise Figure	3.5 dB	3.5 dB
IF Center Frequency	18.4 MHz	18.4 MHz
Voltage Gain (linear mode)	88 dB	90 dB
Gain Control Range (linear mode)	>60 dB	>60 dB
Dynamic Range (log mode)	65 dB	65 dB
Video Output (log mode)	see curve	see curve
Local Oscillator Frequency	55.2 MHz	55.2 MHz
Recommended Local Oscillator Power	+7 dbm	+7 dbm
Input Power Requirements	115V AC, 60 c,s single phase	
Minimum Discernible Signal	-106 dbm	-106 dbm

*When measured from 50Ω source

Figure 8. Typical Data for a Receiving Channel

for display on two 5-in. cathode-ray oscilloscopes that serve as sources for the optical combiner which stores the return display on film.

The data-processor generates the modulating pulse for the exciter unit, triggering signals for the framing camera, the clock light and the sweeps of the oscilloscopes.

The data-processor also performs a validation or signal recognition function which determines whether the signal is a meteor trail echo or an isolated interference pulse. This is accomplished by counting a preset number of radar returns at a constant range during a preset time period. If the desired number of returns are obtained, it is assumed that they are produced by a meteor trail; and the oscilloscope sweeps are triggered, the camera takes a picture of the data displayed, and the film is advanced preparatory to the recording of the next meteor trail return.
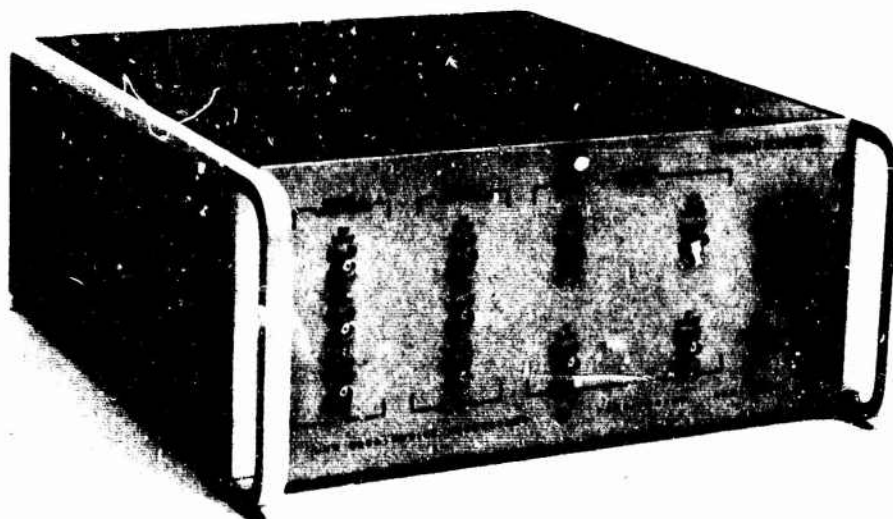
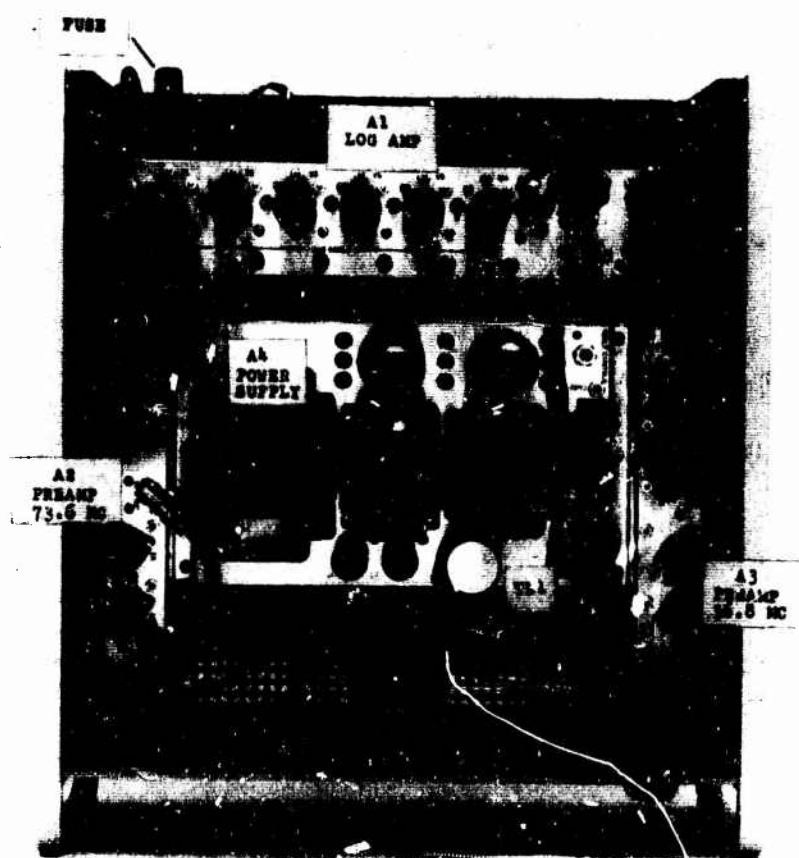Figure 9. Front View, Receiver Subsystem

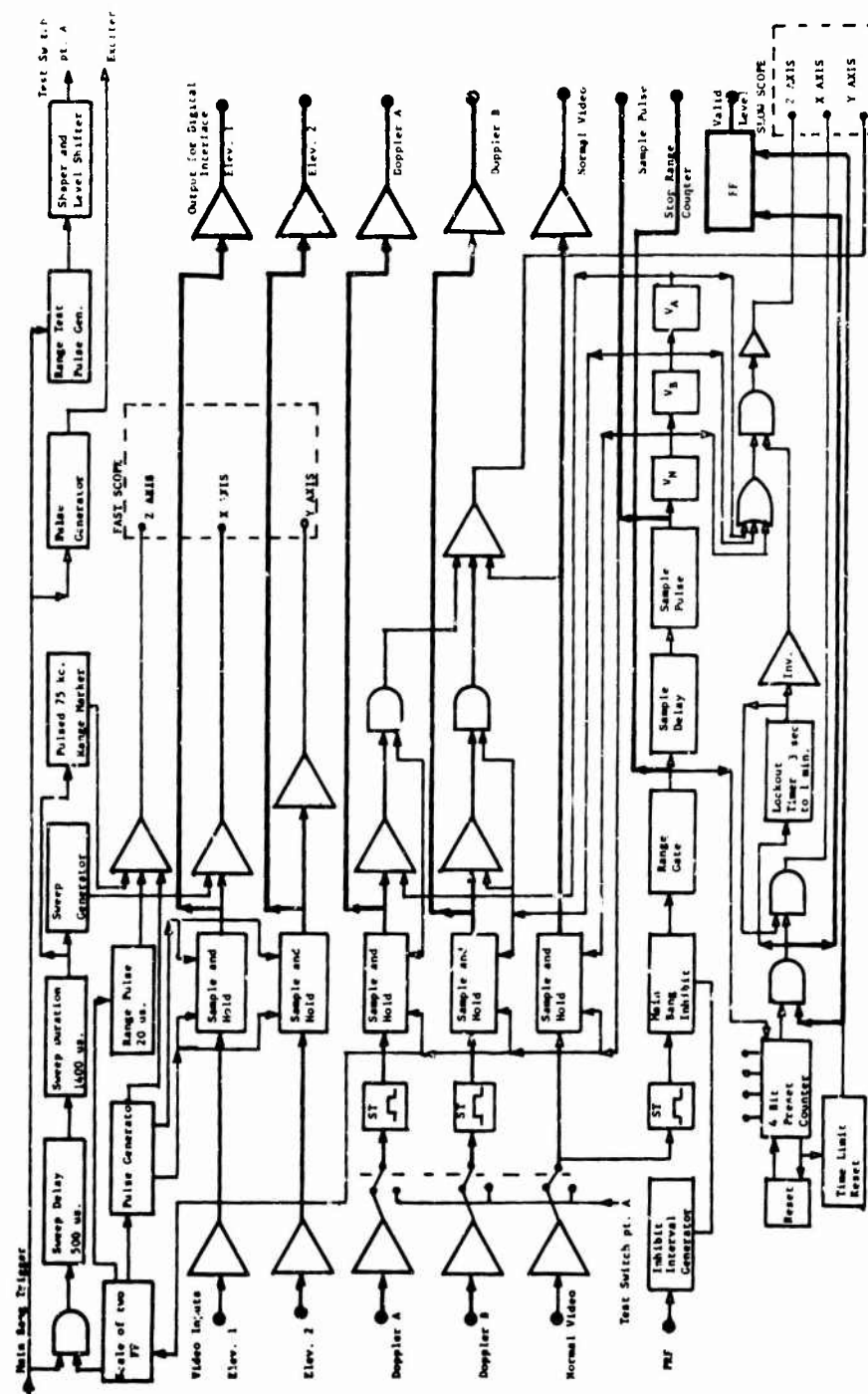Figure 10. Top View, Receiver Subsystem - Cover Removed

Figure 11.

Generally, in Figure 11, the connections to the receiving and transmitting subsystems are shown on one side of the figure, while connections to the interface electronics between the radar and the computer as well as the display oscilloscopes are shown on the opposite side. The components outlined in heavy lines are all that were required to provide inputs for the digital interface. The optical combiner inputs are the images on the faces of the two Dumont 410BR 5-inch cathode-ray oscilloscopes with self-contained sweeps, amplifiers, and power supplies. The faces of the two oscilloscopes are projected side by side on one frame of a 16-mm movie film by means of mirror arrangements designed for this purpose. Figure 12 shows a typical frame of data. A twenty-four hour clock and two light bulbs (which are either on or off to indicate antenna direction) are also projected on the edge of the 16-mm film frame by another system of mirrors. One oscilloscope is designated the slow-scan scope. The sweep speed is 0.5 second but it can be readily varied. One of the vertical deflections of this oscilloscope display shows the amplitude variation of the return from the trail as a function of time. From this trace, the bottom one in Figure 12, the decay rate is obtained as a slope, since the signal input is a logarithmic function of the signal strength. The air density may then be computed utilizing some assumptions concerning the diffusion of the electrons in the underdense trails.

Two other traces are time-shared on the slow-scan scope to display the Doppler data. This data is converted to a binary form in the data-processor and is displayed as two square waves at the top of the scope face as Doppler A and Doppler B traces. The frequency of either square wave is the Doppler frequency shift of the trail return, and the phase difference between the two square waves determines whether the trail is approaching or receding from the radar.

The other oscilloscope is designated as the fast scan-oscilloscope. The sweep for this scope is generated at the repetition rate of the transmitter and is intensity-modulated with range markers at 4-km intervals. The output from the DDR channel is converted in the range channel of the data-processor to an intensity-modulated signal, which is presented on the same trace as the range markers. Therefore, the range of the meteor trail is easily read from the film. The display of the elevation angle data is time-shared with the range trace by displaying the outputs of the two elevation antennas as vertical and horizontal amplitude deflections respectively. As the amplitudes of the echoes decay, a straight line of dots is displayed, and the slope of this straight line is a direct measure of the ratio of the two elevation angle outputs.

As described above, the data-processor, including oscilloscopes, optical combiner, and camera, were used for filming the data, which would lead to objective rules for recognizing signals. The equipment up to this point is an implementation of the Greenhow technique and should permit the duplication of the data obtained at Jodrell Bank.

Figure 12. DST, 9 August 1964, Lexington,
Mass., 36.8 MHz Log Elevation Angle,
Time: 0.5 sec, 200 pps, 100 μsec

The filmed data was processed manually and the calibration curves for the elevation angle antennas were used to obtain computed heights for the meteor returns. A comparison with the independently measured densities used in defining the various standard atmospheres, which extend to this altitude, helped to point out inconsistencies, circuit problems, and the effects of noise in the received signals.

The next step was to prepare the data for entry into a small general-purpose computer for accomplishing real-time data reduction. The design plan had prepared the way for this step, and only minimum modifications of the set were required to derive the required interface for insertion of the data from each return into the computer. Figure 13 is a block diagram of the interface electronics. The normal video and the two elevation angle outputs are amplified in the interface electronics and fed to an analog-to-digital converter which converts the three signals into three seven-bit binary words. These words are then fed to the computer buffer input register. The slant range to the meteor trail is obtained in digital form by starting a counter with the transmitter trigger pulse generated by the computer. This counter is driven by an accurate train of clock pulses at 124 kHz. The leading edge of a valid return from a meteor trail stops the count and the eight-bit binary word in the counter is read into the computer at the appropriate time. The Doppler A and Doppler B signals in the data-processor are already in a form which allows them to be inserted directly into the input buffer in the form of one-bit words.
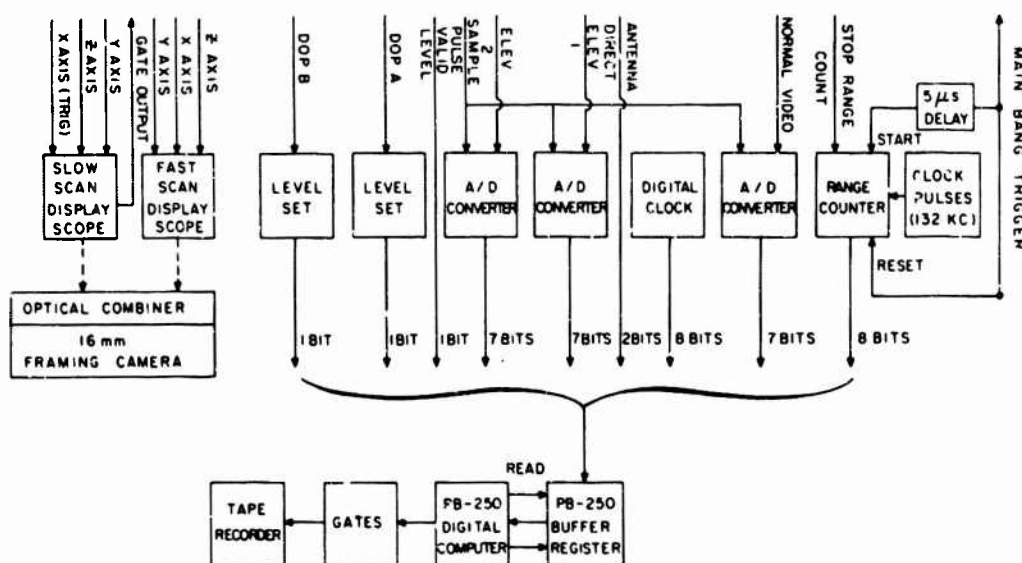
Figure 13. Interface Electronics and Photographic Display and Recording

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In order to compare the film data with the computer data and also to maintain a record of the time at which a given trail occurs for statistical purposes, it is necessary to insert a time word into the buffer register. This is done with a digital clock which has a 24-hour capability and is synchronized to the 60-cycle power line. This provides sufficient accuracy to identify individual trails and to sort the trails for statistical processing.

The antenna direction is available at one of two voltage levels which are sensed by the computer. The interface electronics also contains a flip-flop which is set by the validation circuit in the data-processor. When the computer senses a change in the validation level output of this flip-flop, the information from each pulse for the next one half second is stored in the memory of the computer, after which the computer proceeds to run through the data-processing program.

The computer controls the triggering of the transmitter and is programmed to produce triggers approximately 2 msec apart. These triggers are delayed an appropriate amount and then used to generate the modulation pulse for the exciter in the transmitter. After the computer senses a validation and stores half a second of data, it stops producing triggers, processes the data and either records the results on magnetic tape in IBM digital format or prints them on the Flexowriter. Upon completion of the output program, triggers to the radar are re-initiated.

At the beginning of the 2-msec interpulse period, the range counter and the sample-and-hold circuits preceding the analog-to digital converter are reset.

5.6 Data Output

The display oscilloscopes present the data on film for manual analysis. Some further examples of the data are shown in Figures 14, 15, and 16, with the sweep moving from right to left in the photographs.

In Figure 14 the elevation angle appears in the top half of the picture as a sloping straight line with a cluster of points above the line. As the signal strength decreases with time, the contribution of noise becomes a larger portion of the signal on each succeeding pulse. The antenna pattern of each elevation channel picks up differing amounts of noise because of the lobe orientation at 40° and 60° respectively. Thus the noise-affected returns rise above or fall below the line depending on the source of the noise or interference. The most reliable data are those shortly after trail acquisition while the signal strength is relatively large.

In the lower part of the frame the normal video amplitude (lowest trace) rises and then falls, indicating that the trail was not underdense. The Doppler trace is insufficient to measure the wind speed because a complete cycle was not obtained. This trail was unable to provide any wind or density data.

Figure 14. DST, 9 August 1964,
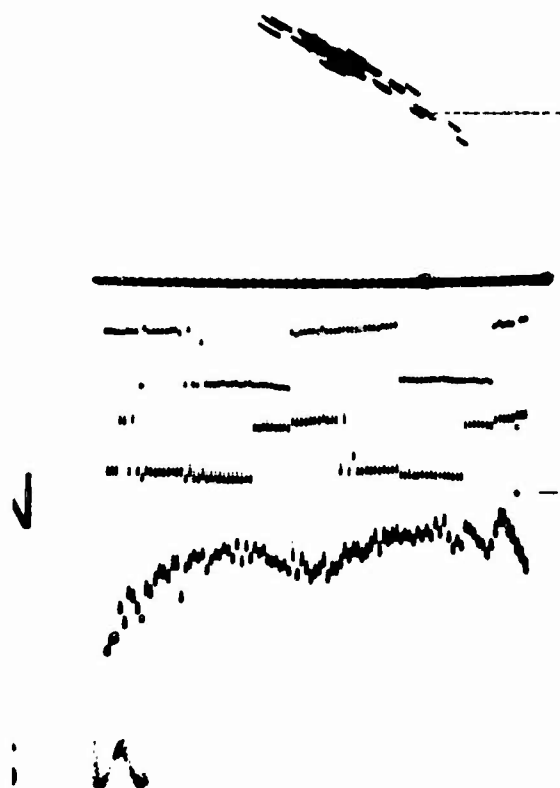Lexington, Mass., 36.8 MHz Log
Elevation Angle, Time: 0.5 sec,
200 pps, 100 μsec

Figure 15. DST, 9 August 1964,
Lexington, Mass., 36.8 MHz Log
Elevation Angle, Time: 0.5 sec,
200 pps, 100 μsec

THE FLOOR. I don't see the Doppler in Figure 14.

MR. RAMSEY. The Doppler was starting to be recorded, but the trail didn't last long enough, and it was interrupted by noise. It may have been a fairly low Doppler shift, but it never got a chance to write.

THE FLOOR. What is being recorded on the scope face in writing the Doppler? Is it phase shift?

MR. RAMSEY. Yes, I am sorry. We are recording the Doppler shift at 36 MHz due to the motion of the ionized trail. All we are doing is simply reconstructing the zero crossings based on the offset frequencies. I think I can show you this better in Figure 15.

In Figure 15 the range is closer, the signal strength higher, and some noise is evident in the elevation angle trace. The Doppler measurement of the wind is

usable, but the decay curve of the normal video indicates that this was an overdense trail and the slope is not suited to a density measurement as it stands. Wind and height data could be obtained from the trail, but not density.

Figure 16 illustrates a return from an overdense trail with two slopes. The Doppler shift indicates that the radial component of the wind was less than 10 m/sec.

These examples illustrate some of the problems caused by operating in a noisy location, and some of the assumptions necessary to interpret the signal in terms of density and validity. Knowledge of the three-dimensional antenna patterns and the azimuth of the signal is needed to increase the number of signals giving usable density data.

In addition to the display on the oscilloscopes, an output is obtained from the computer on an incremental Magnetic Tape recorder in standard IBM format. The data recorded on a pulse-to-pulse basis is summarized in the following format as shown in Figure 17.

METEOR TRAIL OBSERVATIONS
DIRECTION N   TIME 207

RANGE	EL.A	EL.B	DA	DB	VIDEO
123	0	6	1	1	48
123	86	78	1	1	49
123	0	6	1	1	50
123	85	80	1	1	51
123	0	6	1	1	50
123	80	74	1	1	50
117	0	6	1	1	11
123	91	81	1	1	50
123	0	6	1	1	50
123	91	91	1	1	50
124	0	6	0	1	48
123	84	86	0	1	47
123	0	6	0	1	48
123	96	89	0	1	49
123	0	6	0	1	49
123	74	73	0	1	48
123	0	6	0	1	51
123	88	86	0	1	51
124	0	6	0	1	52
123	83	80	0	1	49
123	0	6	0	1	50
123	84	77	0	1	50
123	0	6	0	1	50
124	84	82	0	1	47
123	0	6	0	1	48
123	89	83	0	1	50
123	0	6	0	1	50
123	85	85	0	1	49

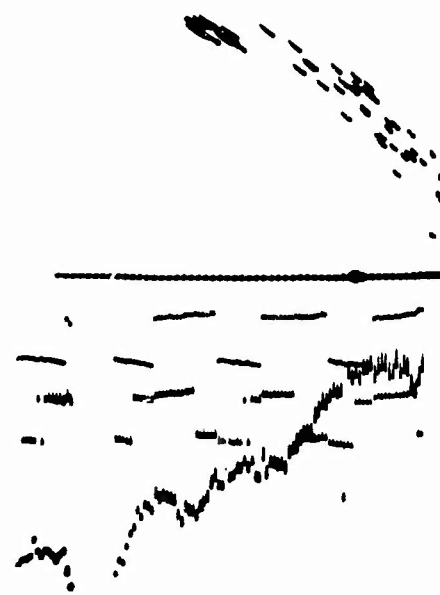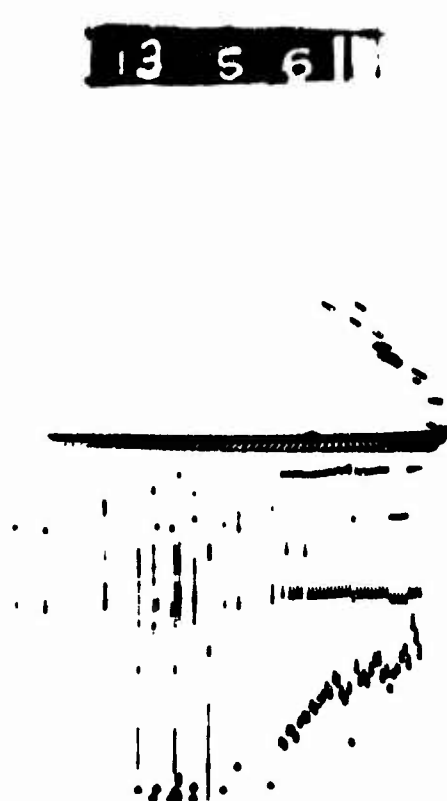Figure 16. DST, 9 August 1964, Lexington, Mass., 36.8 MHz Log Elevation Angle, Time: 0.5 sec, 200 pps, 100 μsec

Figure 17. Flexowriter Printout of Real Trail July 1966 (A.A. Barnes)

The range is shown in units of time. The elevation signal strength is recorded for each channel of the split beam system. The Doppler channels are given next; and the strength of the normal video signal, in units which can be converted to decibels, is given in the last column.

Time and the antenna set in use are recorded giving all the information needed for the computer to carry out the program for deriving the desired meteorological information.

THE FLOOR. Is that a real trail?

MR. RAMSEY. Yes, Figure 17 is a real trail. The first column is the range. It is a count which, with proper calibration, gives you the range. The second column is Elevation A and the third is Elevation B. You will notice that we get elevation information every other pulse. The time is 20.7 or 2042 hours.

5.7 Computed Data

Upon detection of a valid pulse, the computer program stores the radar information on a pulse-by-pulse basis in the computer. After 246 pulses are stored in the computer, the following information is computed from this data:

> Range in kilometers
> Elevation angle in degrees
> Normal video amplitude in decibels
> Normal video decay rate
> Height of return above the surface (corrected for earth
>   curvature)
> Wind component in meters per second
> Time in hours and tenths

6. SUMMARY

The technique implemented in this meteor trail radar set has shown the capability for unattended operation, producing raw data required for research purposes. There are a number of areas where improvement is required before the internal consistency and the meteorological value of the information can be assessed.

(a) The first area where improvement is needed and is possible is the operation of the set in a less noisy environment. When noise and even-pulsed carriers appear on the operating frequency some of the returns are rejected by the computer program but many more are just lost in the noise. The threshold levels for validation are raised to keep the set from continuous triggering. Higher transmitter power may help with this problem by raising the strength of the returns, but it is an expensive solution and may not be permitted because of the increased strength of harmonics which might cause a problem with the computer operation adjacent to the transmitter. Figure 18 shows the equipment in its trailer with the receiver
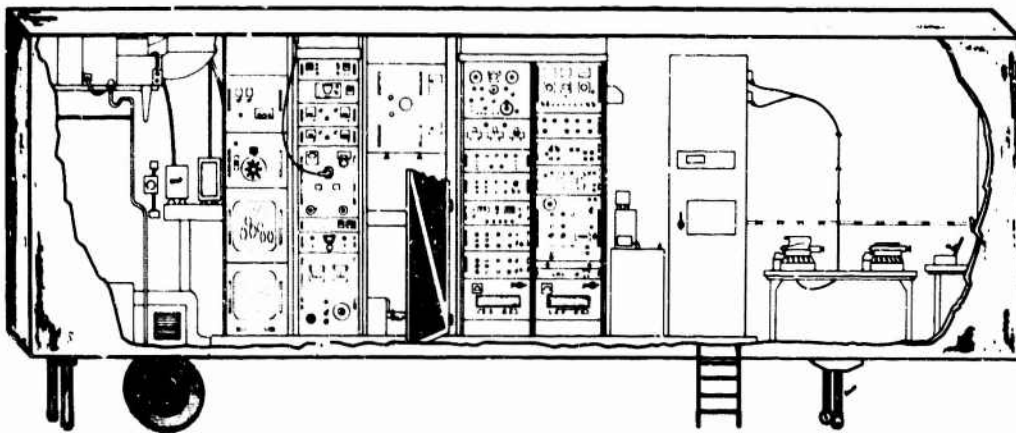
Figure 18.

subsystem between the transmitter and the computer; but there is reason to believe that a major increase in transmitted power, such as might be required to improve the returned echoes significantly, would make the computer operation marginal.

(b) The height accuracy is not good enough for meteorological purposes. Some of the trouble is undoubtedly caused by noise pulses adding into the signal, particularly the lower antenna beam which is susceptible to ignition noise. Considerable thought has gone into antenna systems which might improve the accuracy of the elevation angle.

If any trails could be observed with an antenna system which looked directly overhead, underdense trails would give a density, while the range would be very close to the correct height. Such a system, where the height and density would be used to establish a scale applied to all trails not overhead to give their height, might turn out to be more accurate than any angle measuring system. Such a supposition depends on a vertical-pointing antenna with either a sharp null overhead or a narrow beam, and on the occurrence of trails with specular reflection higher than about 80° above the horizon.

(c) A small, fast, random access computer is needed in an operational system to permit a faster PRF to provide enough data to identify the Fresnel zones as a necessary condition for valid signal decay data for density determinations. Equally important is the ability to exercise logical decisions on a pulse-to-pulse basis rather than on the basis of the whole trail. The use of a machine like the SDS 920 should provide capability for increased range gate resolution, the recognition of noisy signals, and the use of more realistic selection of valid data.

Our experience has shown that, in general, it is more practical to utilize the reliability and speed of the small general-purpose computers which are now available than to design, build, and test special-purpose hardware for implementing the same logical decisions.

(d) The stability of the calibration of the whole analog portion of the system needs to be validated automatically at regular intervals. Variations of gain in the various channels require frequent calibration to hold the level of accuracy the data demands. If gains change, it is not now possible to look at the data and determine when the changes occurred. Hourly injection of known signal levels would be very desirable in the next generation of devices.

(e) A parametric system study of power level, frequency of the transmitter, bandwidth, pulse width, number of returns per hour, false returns from aurora, ionosphere or other sources of non-valid energy at the working frequency, and possible solutions to the height accuracy requirement are certainly in order.

This set has been a source of data for education, for developing processing rules leading to automatic data-reduction both on-line and off-line, and for the measurement of wind data at meteor trail levels. The data accuracy is not yet sufficient to investigate the assumptions which make up the basis for density determinations or the gathering of meteorological data in layers of 2 km or less.

The authors would like to thank Paul Donato and Richard Jordan of Indatacon and Dr. Barnes of the Air Force Cambridge Research Laboratories for their help.

MR. MYERS. Thank you, Bill. Do we have any questions?

THE FLOOR. How about the 1.5 MHz rf bandwidth; that is, of course, far in excess of what you need to have as optimum. Is there any reason why you should use such a wide bandwidth?

MR. RAMSEY. No, primarily it was that the receivers that we were interested in were so specified.

THE FLOOR. I am using the very same equipment. We have 150 kHz, and we are getting good results. There is no reason why you should go to more than 1 MHz.

THE FLOOR. In your consideration of trying to look overhead to get accurate height, you would be plagued by your beamwidth, and you would have to get a very narrow beamwidth. You would then lose your horizontal wind motion.

MR. MYERS. The idea would be to use this only as a density vs height calibration. If you had one antenna with a null directly overhead, and a second antenna with a hemispherical coverage, and if you received a signal on the second antenna and not on the first, you would know it was overhead. You can use this as a typical example and try to be sure that you are looking very nearly overhead. Then, if you get a return from the trail at 86 km and had a similar density associated with that, every place you got that density you could assign a height of 86 km. That is

a transfer function from height to density. This is the sort of thing that you are led into. We have not tried it.

DR. ELFORD. It is not meteors you are going to see straight over; it is something else.

MR. MYERS. If there were any meteors, even a few, this would help to tie down some of the other problems with this particular single station technique.

DR. BARNES. I see your objection. Actually we would not look directly overhead, but in a cone where the variation of height for a constant range is small. This is what we are shooting for, and we recognize that there are not many meteors leaving trails parallel to the earth's surface.

THE FLOOR. You are still working on sines and cosines. You are also going to have the problems of irregularities in trails giving trouble.

MR. MYERS. I don't know of any other type.

THE FLOOR. By which method do you recommend measuring exponential decay?

MR. RAMSEY. Well, we have devised a normal video system set up to give the logarithm of the signal input levels, and essentially what you get out is a linear variation corresponding to the power returned from the target. Now, this factor is ideally displayed from the standpoint of trying to lay a line on it, automatically giving you a rate of change in decibels over a certain amount of time. You have to prove it was an underdense trail, and then, by plugging this into proper equations and knowing absolute temperature, you can get an absolute density.

THE FLOOR. Very often you have superimposed on the exponential decay a distortion of the echo, and it is not very easy to process it out.

MR. RAMSEY Yes, admittedly, it may have been fairly crude. In the computer processing, what we did was to look at the pulse-to-pulse returns and then try, first of all, to determine a straight line. If there were a straight line occurring, and we had this over a segment of the trail which we felt was reasonable, we used it to determine the decay if the variations of the rest of the points from the straight line were not great.

THE FLOOR. If you had a sinusoidal amplitude, it would be.

MR. RAMSEY. Yes. This was crude, and Dr. Barnes may discuss how well this worked. We felt at least this was a start.

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Greenhow, J. S. and Hall, J. E. (1960) Diurnal variations of density and scale height in the upper atmosphere, <u>Atmosph.</u> <u>Terrest.</u> <u>Phys.</u> 18: 203-214.

Myers, Robert F., (1963) The Ocean Breeze and Dry Gulch Diffusion Programs, Volume II, Research Report, AFCRL-63-791 (II), Air Force Cambridge Research Laboratories, Bedford, Mass., pp. 23-46, edited by D. A. Haugen and J. H. Taylor.

IV. The Stanford University System, I

Prof. Allen M. Peterson
Stanford University
Stanford, California

I will be the lead-off speaker and Bob Nowak will be the leading speaker. I want to quickly point out that we are still in the construction stage, although we are recording wind data at times when it can be done simultaneously with the Air Force Cambridge Research Laboratory measurements. We have been working towards a system but primarily working on techniques which will enable us to better record in a form suitable for digital processing. I can remember the first work with meteor winds at Stanford back in 1950; and that it was exciting for the first few weeks. After that, because of the data processing, it became much less exciting and we found other things to do.

I never lost my interest in meteor winds, and when recently it was found that suitable digital-stepping tape recorders were available, a system was planned in which the processing could be more or less automatic. We are very pleased to have had the support of AFCRL in the design of this system.

In designing our equipment we necessarily made compromises so that the data format would be suitable for digital recording and processing. Further constraints were imposed by the available budget and by the thought that we wanted an equipment whic' would be possible to duplicate, so that winds could be measured at a number o locations simultaneously. Keeping this in mind we looked for ways that would keep transmitter costs down and simplify the operation of the equipment. Use was made of equipment that was left over from the IGY. The available transmitters were capable of about a kilowatt of average power. While there was some

capability for higher peak-power, we decided to limit the peak-power requirements since our experience has shown that the reliability of the system could thus be improved. Cw operation was a natural candidate and was in fact used successfully many years ago at Stanford. Now, however, it is virtually impossible to use cw in our area because there are so many airplanes in the air. Several airplane Dopplers are observed simultaneously at almost anytime.

Instead of cw, we have designed a simple form of pulse-Doppler system with high duty cycle operation. A 1-msec pulse is transmitted at a 300 pulse per second rate. This results in a 2-msec interval in which to measure the meteor echoes. This coherent pulse system has permitted the Doppler measurement to be made almost as simply as would a cw system and eliminates the nearby airplanes.

In order to measure range more accurately, while at the same time maintaining our high duty cycle, we have investigated "coded-pulse" techniques. At present a 28-bit pseudo-random phase-coded pulse train is used. A 10-μsec bit length is used. With suitable processing the resultant 280 μsec coded-pulse gives 10 μsec equivalent range resolution. The system sensitivity achieved by this low peak-power coded-pulse train is equivalent to a 70-kW single 10-μsec pulse. The completed system will use a composite pulse, with half the power in a simple long pulse and half of it in the phase-coded range pulse. We will have then, an effective 280-μsec pulse with half the power used for the narrow band Doppler measurements and the other half used for the wide-band ($\approx$ 200 kHz) ranging signal.

Thus we are aiming for a low peak-power compact system with about 500 watts of average power, which will result in several hundred digitally recorded meteor echoes per hour during daily peak periods. We also hope to do this in a fashion that will be inexpensive to duplicate, so that synoptic data on winds can be obtained at a number of locations. Mr. Nowak will discuss the present status of this digitized system in some detail.

V. The Stanford University System, II

Robert Nowak
Stanford University
Stanford, California

At Stanford, meteor work resumed in 1963 with a simple coherent pulse system transmitting and receiving fairly long pulses at the same site and measuring winds alone. No density measurements were made and the height of the measured winds was not determined.

The difficulty in measuring winds from Doppler shifts is that not only the frequency difference between transmitted and received signal has to be determined, but also if the received signal has a higher or lower frequency than the transmitted one, i.e., if the trail is approaching or receding must be known.

In our system, this problem is solved by comparing the received frequency not directly with the transmitted one, but with this latter one shifted by a small amount (at first 30, later 40 Hz). Thus, a stationary target would give a Doppler output of 30 Hz, receding trails would lead to a lower frequency, approaching ones to a higher frequency.

Figure 1 shows a typical record of wind data. These were recorded in analog form on magnetic tape, at slow speed. Playback was on a "Rayspan" analyzer (essentially a parallel bank of filters), with the tape speeded up about 80 times. The heavy line in the middle is a 30-Hz signal left leaking in from the shifter to establish a reference line. For easier data reduction, the display would be expanded considerably. Echoes appear as fairly wide lines, because the signals, with their short duration, have a broadened spectrum.
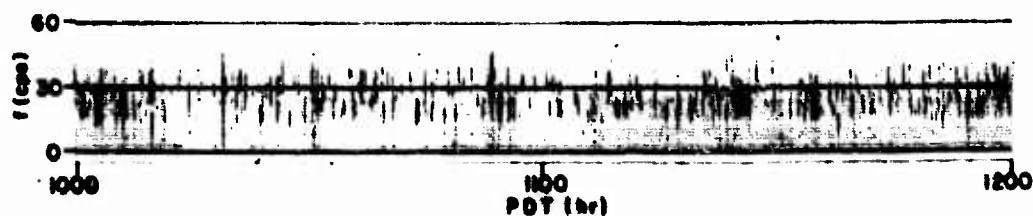
Figure 1. Typical Rayspan Output of Doppler Signals

There are two advantages in this method of Doppler recording. The first is that only one channel is needed to display both magnitude and direction of the Doppler shift. The other is that, because of the fairly high frequencies involved, several half-cycles of the Doppler signal are present during the short meteor echo and can be averaged for good measurement accuracy.

Shown in Figure 2 are measurements of winds made last year with this tech‑nique. These are mean hourly winds, taken over the entire height range of the echoes, and totalled over the measurement period of about 18 days. The daily variation pattern of the hourly mean winds was closely repeated in this period. The data suggest a nearly constant wind vector, rotating clockwise with a 12-hour period. Two explanations are possible for these results. One is that there is actually such a behavior of the mean winds. The other is that since winds in the meteoric height region are highly stratified the change in direc : n might be one in space rather than in time and that through the daily variations of meteor radi-ants, different height regions are sampled during the day. To decide between these alternatives, the altitude of the reflecting trails has to be known.

To determine it, we started recording echo amplitude; from its decay the altitude can be obtained. Recording was done on analog tape with subsequent A/D conversion and computer calculation of the decay slopes. Figure 3 shows a plot of several echoes, with amplitude as obtained from the digitized data, and the Doppler signal. Total duration of each plot is 0.6 sec and time information is printed out with it. The three records on the upper right are consecutive and probably show one echo from a long enduring overdense trail. The record on the lower right seems to show several individual underdense echoes. Closely spaced echoes were observed fairly often during shower periods.

Figure 4 shows an example of the data obtained from these measurements in the north direction. Height was determined from echo decay with the parameters of the 1962 US Standard Atmosphere. Unfortunately, not too much about the fine structure of the wind can be deduced from these results but a general shear is apparent.
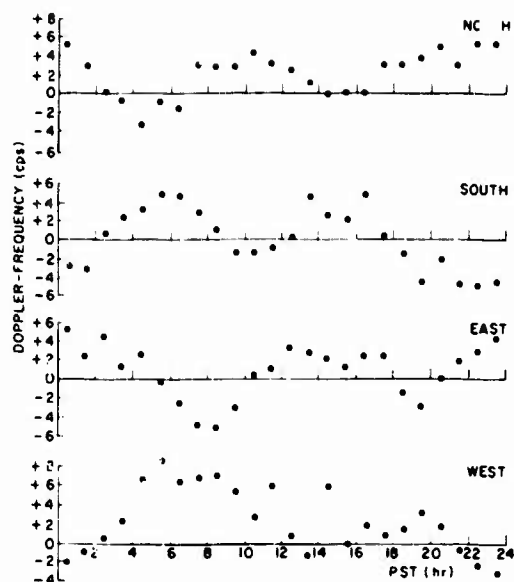
Figure 2. Mean Hourly Winds, Averaged Over Four Days in Each Direction. Data from April 20 to May 7, 1964
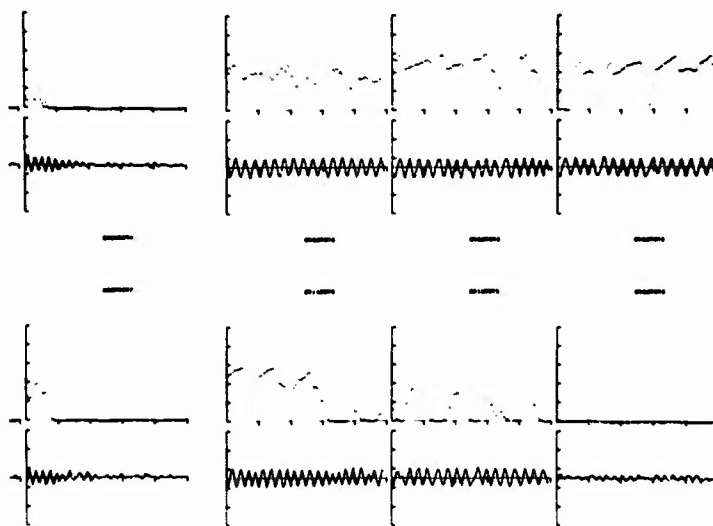

Figure 3. Typical Records, Converted from Analog Recording to Digital Form and Plotted; Data Taken August 1965
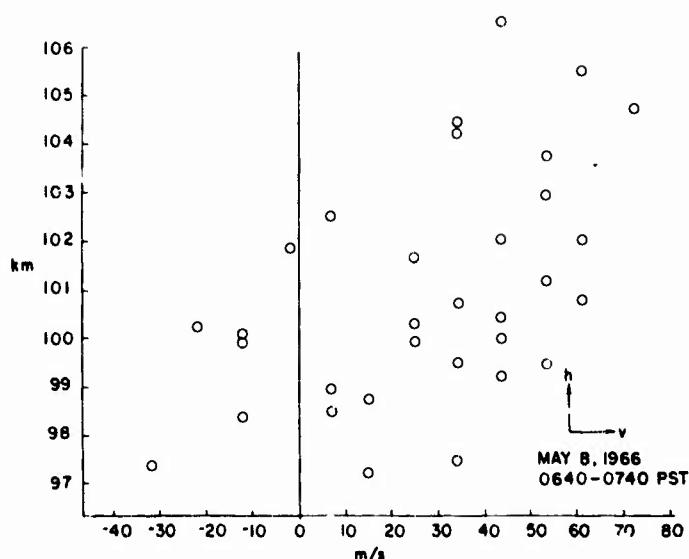
Figure 4. Winds Recorded in North-Direction During
One Hour, With Heights Established from Echo Decay

The antenna used was a 3-element Yagi with a beamwidth of about 60 degrees.
Thus, some errors are made by observing winds that are not exactly in the direc-
tion of the beam axis, but nevertheless assuming that all echoes are from this
direction. The antenna beam has a low elevation angle so that the measured
(radial) wind component is almost equal to the horizontal wind.

THE FLOOR. You cannot see any S-shape in the profile because your height
interval is too small. You would need at least twice your present height range.

MR. NOWAK. We are actually planning to study a bigger height range in the
future. We also expect a better data rate in the future with the new system in which
all parameters have been optimized. Let me give now a short description of this
latest system of ours. It has just been completed and awaits calibration; no data
have been taken with it yet.

Our aim was to provide a low cost meteor patrol for the measurement of
winds and densities as a function of height that would be accurate but simple enough
to provide the base for a network of stations for synoptic measurements, operating
unattended. Parameters to be measured are Doppler shift, echo decay, and height;
the last is determined by range and elevation angle. To obtain the Doppler shift
we are using the same technique as before, which I just described.

For the measurement of winds, which are expected to be predominantly hori-
zontal, one would like to use an antenna with a low angle of elevation, since here

the measured radial component is almost as large as the true wind and errors are minimized. At low elevation angles, however, height cannot be determined accurately enough since the elevation angle would have to be measured with an impossibly high precision. If, for example, one wants to obtain the height of the echo to within ± 2 km at an angle of 25°, this angle would have to be measured to within ± 0.5°. If height is determined at high elevation angles, requirements on the accuracy of the angle measurement can be relaxed, but the detected radial motion of the trail will be much smaller than the true horizontal one and large errors will be made in the wind measurement.

This morning, Mr. Myers pointed out a solution to this problem. We measure echo decay as a function of height at high elevation angles and obtain a "calibration curve" for the decay-height relationship. This is used for the wind measurements in which only decay is recorded.

The method for recording data posed a major problem, since data are to be reduced on a computer. An attempt to develop a digital network for data-processing at the station itself failed, and it was finally decided to record data, in digital form, on magnetic tape. The main advantage of direct digital recording is its low cost. (Since, for computer analysis of the data, these have to be presented in digital form, any analog record would have to be digitized. Our experience with the old station showed that this is very expensive.) A "Kennedy" incremental recorder, which can record up to 500 6-bit characters (in IBM-format) per second, was chosen for this task. Since our radar has a pulse repetition rate of 300 Hz, each returned pulse can be recorded in real time, but with the limited resolution of 6 bits. The quantization error introduced by this recording procedure will be analyzed tomorrow.

One of the main parts of the system that has just been completed is the ranging unit. As Dr. Peterson mentioned, we are using a coded pulse in a so-called pulse-compression technique. During the radar pulse, the signal is phase-modulated between two values 90° apart, in a 28-bit code with a 10-μsec bit length. A specific reference phase does not have to be maintained since the received signal, which is compared with the transmitted one, has an unknown phase shift anyway, through its round-trip time. Figure 5a shows that the signal can be taken as the sum of a constant phasor and one switched in phase by ± 90°. Since 280-μsec pulses are used with a PRF of 300 Hz, the resulting spectrum shown in Figure 5b leads to a narrowband (7-kHz wind) spectrum, which can be filtered out and used for amplitude measurements since it simply corresponds to a conventional, 280-μsec-long pulse. The switched phasor, V_{11}, V_{12}, has a spectrum that is about 200 kHz wide, corresponding to the bit length of 10 μsec in the code. Ranging is accomplished with it. The 28-bit code was carefully chosen to present an auto-correlation function with only one narrow major peak. As the echo comes in, its phase is detected

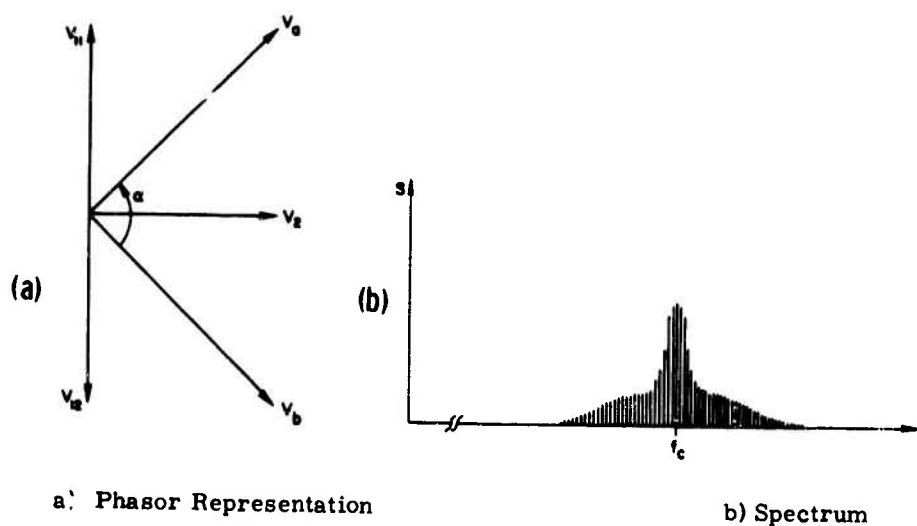a) Phasor Representation   b) Spectrum
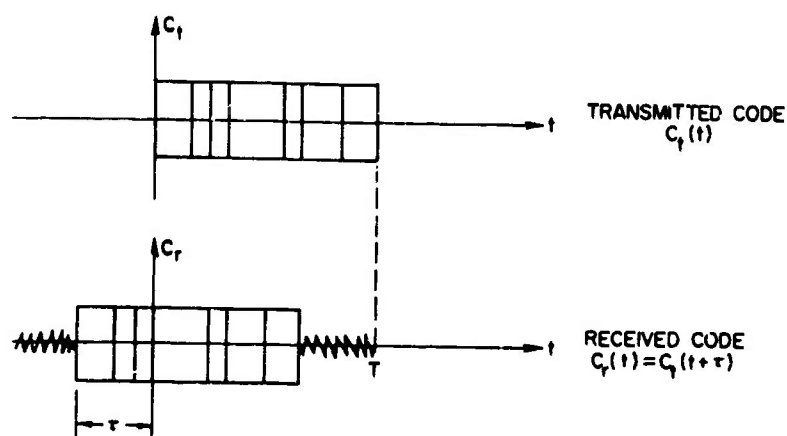
Figure 5. Composite Signal

Figure 6. Crosscorrelation Between Transmitted and Received Codes

and, in a shift register, compared with the transmitted code which is wired into the register. Figure 6 shows the correlation between transmitted and received codes. As the received signal is shifted in, it is correlated every $10\,\mu$sec with the transmitted code, i.e., in all the code bits the expression

$$r = \frac{\text{number of agreeing bits - number of disagreeing bits}}{\text{total number of bits}}$$

is formed (the total number of bits here is 28). For noise, $r = 0$ on the average. The moment the code of the echo is lined up with the code transmitted, $r = 1$. For any other position, with the code chosen, r is at most $1/14$. The time of "line-up" of the codes, easily identified by the state $r = 1$, is recorded and yields the range.

The angle of arrival is measured by comparing the signals from two antennas with different vertical radiation patterns, similar to the technique used by the Air Force Cambridge Research Laboratories. The different radiation patterns can be obtained either by choosing different antennas, or by using identical antennas at different heights above ground. In the Stanford radar, both techniques were combined to obtain the largest variation possible of signal ratio with elevation angle. Figure 7 shows the signal ratio of a $\lambda/2$-dipole, $\lambda/2$ above ground and a 2×2 array of $\lambda/2$-dipoles, $3\lambda/16$ above ground, as a function of zenith angle θ. The system was designed for a range in θ between 20 and 40 degrees. (Measurements are desired at high elevation angles so that angle errors are of little influence in the height determination, yet very few meteors are expected directly overhead.) Figure 8 shows a photograph of the actual layout. The antennas are wires (No. 8 phosphor bronze) stretched by weights between wooden poles. After this picture had been taken, a ground-screen was installed by laying out 2-inch chicken wire on the ground.

For the wind measurements, 3-element Yagis, 0.6λ above ground, are used which have patterns with elevation angles of about 25 degrees. In the measurements, the transmitter is switched every hour between one of these antennas pointed west (which can be seen in the background in Figure 8) and another one pointed north.

The shifting of the reference frequency for the Doppler measurement is accomplished in the manner shown in Figure 9. A single-sideband modulation technique is used in which the outputs from two balanced modulators are combined in such a way that one modulation sideband cancels. Since the operation is at a single frequency, no great difficulties are encountered with this method.

Figure 10 shows the block diagram of the complete station. All frequencies needed for transmitter and receiver are derived from a 1-MHz standard oscillator. A compact, inexpensive model (Knight JKTO-81) with a frequency stability of 5×10^{-9} per day was chosen here. Dividing the oscillator frequency by 10 provides a 100-kHz clockpulse for the digital circuits. Thus, the system is phase-coherent

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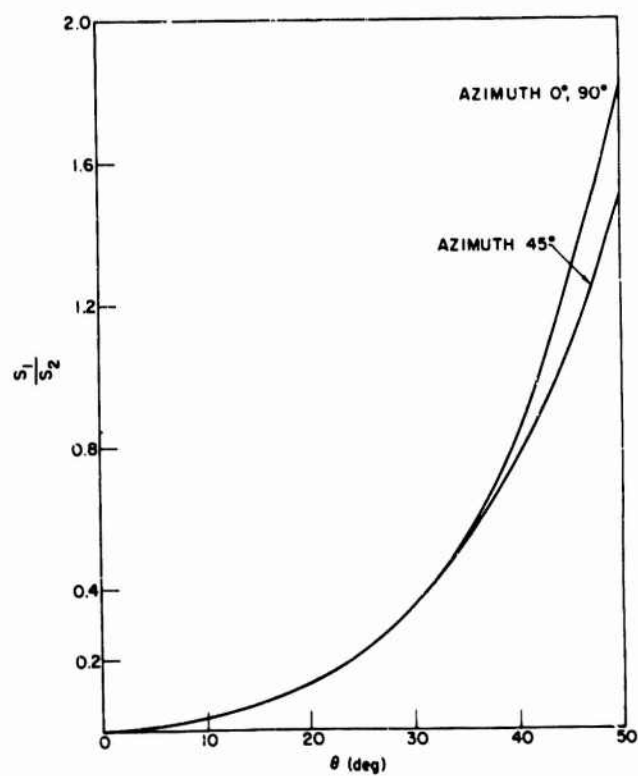

Figure 7. Amplitude Ratio Between $\lambda/2$ Dipole $\lambda/2$ above ground and 2 x 2 array of $\lambda/2$-Dipoles $\lambda/2$ Apart, $3\lambda/16$ Above Ground, as a Function of Zenith Angle θ

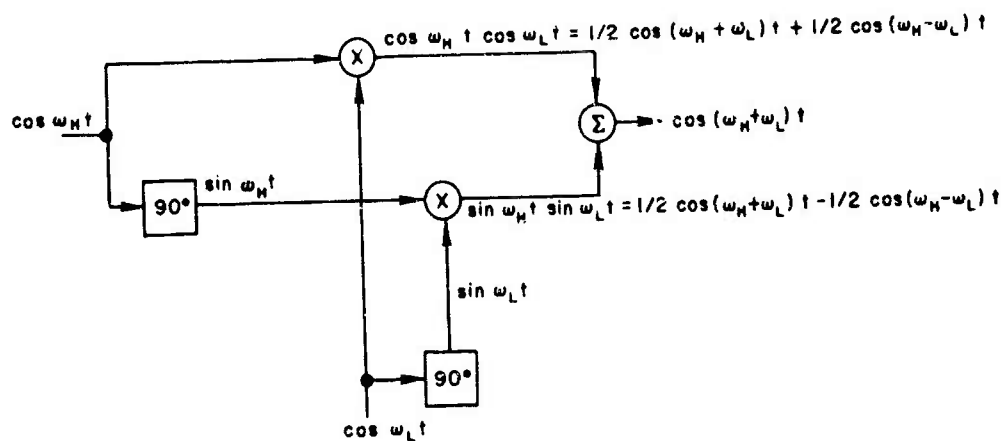Figure 8. Stanford Antenna System

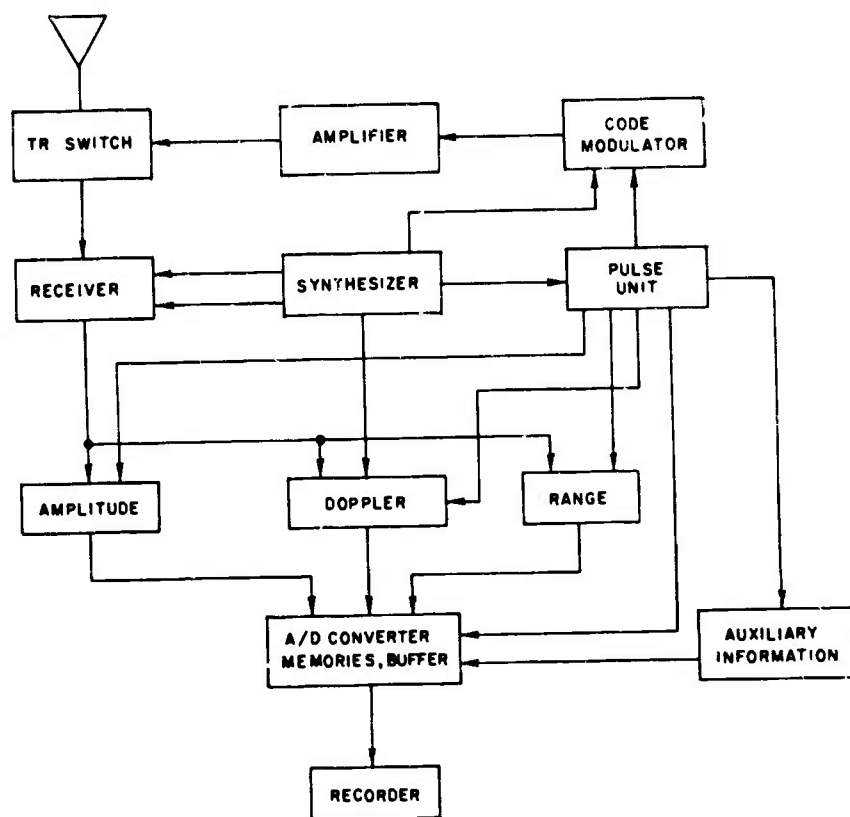Figure 9. Principle of Frequency Shift Technique

Figure 10. Block Diagram of Stanford Meteor Radar

in all its parts except the 30-Hz frequency for the shifting of the Doppler reference, which is generated in an RC-oscillator and is stable to within a small part of 1 Hz. The 30.14-MHz signal for the transmitter, derived with synthesizing techniques from 1 MHz, is phase-modulated with the 28-bit pseudo-random code as described above, and power amplification follows. The final amplifier is a modified "Johnson Kilowatt" model.

Since transmitter and receiver share the same antenna, a TR-switch has to be used. A transistor switch was developed for this purpose. (Note: Later, this switch proved to be too easily damaged by transients and was substituted by a modified thyratron switch.)

The receiver had to be custom made, since it is completely tied in with the rest of the system. Two intermediate frequencies are used: one of 3.14 MHz, the other of 500 kHz. The first IF is wideband (300 kHz). In a phase-sensitive detector, the signal of this IF is compared with a reference signal (3.14 MHz shifted by 30 Hz). From the output of this phase detector, the modulation waveform of the signal is retrieved in a 100-kHz lowpass filter for the purpose of ranging by comparing it with the transmitted code. Beside this lowpass, another one, with 80 Hz bandwidth, is used to obtain the Doppler shift.

In parallel with this processing of the signal phase, the amplitude is measured after passing the signal through the second, narrowband (6 kHz) IF channel. Here, only the center part of the signal spectrum, which is that of a normal, unmodulated pulse train, is preserved. The peak-rectified signal amplitude is converted into digital form.

A squelch is incorporated into the system that starts a fixed-length record (of 210 characters length) whenever the amplitude is above a preset threshold for 4 consecutive pulses. From this moment on, the next 198 amplitude values are recorded in real time. At our pulse rate of 300 Hz, this corresponds to a time period of about 0.6 sec. During this time, five digital values for range and Doppler half-periods are stored in shift registers, to be recorded after the amplitude values. Range information is obtained by starting a counter at preset time in each pulse interval and stopping it when the correlation between transmitted code and received signal is 1. Immediately afterwards, the counter reading is shifted into the memory. Doppler frequency is measured by counting time between successive zero crossings of the Doppler signal.

At the end of the record, time of day is recorded from a simple digital clock. The last character of the record contains service information, e.g., what antenna was used and if the record started less than 25 msec after the previous one ended.

All analog circuits, like receiver, transmitter, and so forth, are fairly conventional and only the digital circuits are somewhat complex. Fairchild integrated circuits are used throughout. With construction on plug-in cards, the complete

station, except for power supplies and power amplification, takes up about 10 inches of space in a normal 19-inch rack.

Until now, operation was with 1-msec long unmodulated pulses with about 1.5 kW peak power. In the Doppler channel, as recorded on the "Rayspan" analyzer from the analog tape, we obtained about 4000 echoes per day. Only a small part of these echoes, however, yields reliable decay information. (Note: Later, with the new system as described above, typical wind data rates were 300 per day. This number includes only those echoes that were classified as underdense and have both wind and decay information. In the overhead measurements of decay and height, about 40 echoes per day could be obtained that yielded all necessary parameters for data reduction.)

MR. MYERS. I think it would be interesting to compare our technologies.

THE FLOOR. Is your code the same one that measures the Doppler?

MR. NOWAK. No, we actually divide our transmitted power into two parts. One part, spread over a wide band, contains the code information for ranging purposes and the other, in a narrow band, is used for amplitude and Doppler measurements.

DR. PETERSON. One could make all measurements on the broadband part alone, but this would be more complicated.

THE FLOOR. ?

DR. PETERSON. Are you worried about the effect of motion of the trail?

THE FLOOR. As far as the Doppler measurements from the code is concerned.

DR. PETERSON. Our Doppler is so small that during one pulse, the Doppler signal is practically constant. A complication would be that we would need a "bin" for every 10-μsec range interval, at the output of each of which the Doppler signal could appear. The equipment would be complex, but the technique is possible, since there are not several targets at once, at different ranges.

THE FLOOR. Are you using a Barker code?

DR. PETERSON. No, it is not.

MR. MYERS. There is a report available on this that we would be happy to see that you get [Final Report AF19(628)-3996, AFCRL-67-0347(SU-SEL-67-046)].

THE FLOOR. There are quite a few echoes that show winds about 100 m/sec. Is your offset of 30 Hz not too small so that some of the fast receding trails might lead to greater Dopplers than this and lead to wrong indications?

DR. PETERSON. We have an offset of 40 Hz now.

MR. NOWAK. With this value, we can handle radial wind speeds up to 200 m/sec (which, at 30 MHz would lead to a Doppler of 40 Hz). Actually, in our past measurements we had winds about 100 m/sec only rarely.

MR. MYERS. Based on the best information we had at the time, we designed our system for windspeeds up to 200 m/sec.

80
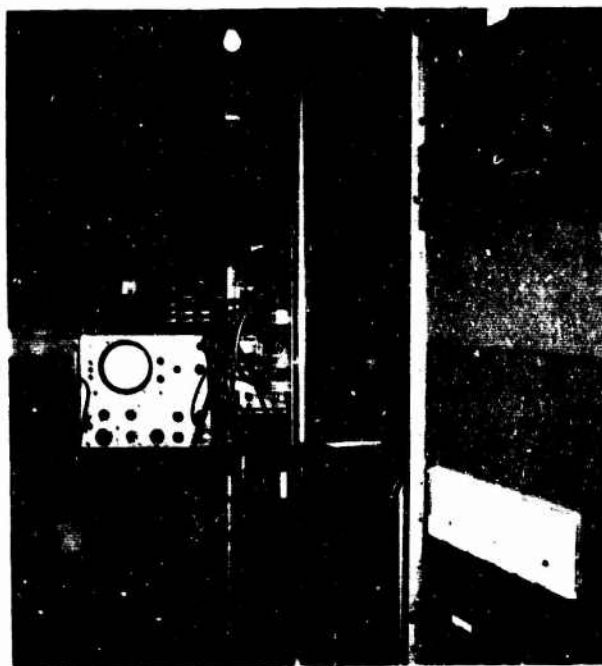
Figure 11. Stanford Meteor Radar. Right rack from bottom: power
supply for final amplifier, power supply for driver, driver. Middle
driver. Middle rack: final amplifier. Left rack from bottom:
Collins R-390 receiver, power supplies, complete station except
power amplification, tape recorder
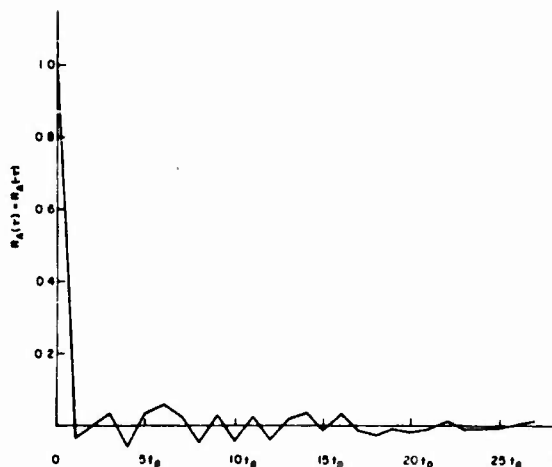
Figure 12. Autocorrelation function of pseudo-random code used to modulate
the radar pulses

THE FLOOR. When the signal fades below the noise and comes up again, are these fluctuations recorded as individual echoes?

MR. NOWAK. When two "echoes" are very close to each other, I disregard both. Unfortunately, during shower periods, I had to eliminate many echoes with this criterion. These seemed to be perfectly good underdense echoes, but they were too close together -- typically 100 msec. When echoes were as far as 500 msec apart and seemed otherwise good, they were processed.

VI. A Continuous Wave Radar for Ionospheric Wind Measurements

Dr. I. Revah
Dr. A. Spizzichino
CNET, France

Summary*

Ionospheric winds are determined from meteor trail drifts, observed with a continuous wave radar. This radar is characterized by a high sensitivity (owing to narrow band receivers) and accurate phase measurements (Spizzichino et al., 1965).

It is a bistatic radar (Figure 1); the transmitter is located at Garchy (France) and delivers a power of 5 kW at the frequency of 30 MHz. The receiving station is at Sens Beaujeu 20 miles West of Garchy. Both transmitting and receiving antennas are corner reflectors with beam axis towards the East at 45° elevation.

Since at 30 MHz directional antennas are impractical, the measurement of the phase differences between the fields received on three different antennas is used to determine the azimuth and elevation of the echo. Three frequencies transmitted simultaneously are used for the distance measurement. The direction of arrival, distance, and Doppler shift are all deduced from phase comparisons, with an accuracy of 0.2°, 300 meters, and 1 meter per second, respectively, for a S/N ratio of 25 dB.

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*Editor's Note: The following is an abstract of the three talks given by Dr. Revah concerning the French system.
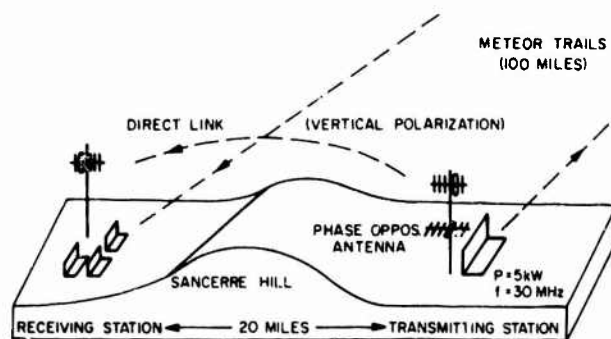
Figure 1.

A first systematic study of meteor trail drifts was undertaken in September 1965 and continued until September 1966 (Revah and Spizzichino, 1966; Spizzichino, 1967).

The high echo rate (50 to 400 per hour) due to the sensitivity of the equipment, made possible a quasi-continuous description of the wind pattern between 80 and 110 km.

From the individual wind velocities measured at different heights and times one can draw a time cross section of the wind as shown on Figure 2. It represents interpolated curves of constant West-East velocity as a function of height and time for November 17, 1965.

The following important features are pointed out:

strong wind shears appearing with gradients up to 50 meters per second per kilometer;

a general downwards motion of the wind profiles, which apparently agrees with the gravity waves theory.

In order to gather more information on the downwards motion, the autocorrelation function p (Δt, Δh), of the zonal component of the wind, has been computed for different lag times, Δt, and height variations, Δh, (Figure 3). The downwards motion is generally confirmed by the high values of correlation for increasing Δt and decreasing Δh. Typical values are:

5 to 10 km for the vertical correlation radius;

1 to 2 hours for the time correlation radius (for constant Δh);

2 to 3 km/hr for the descending speed of the profiles.

The descending motions have already been pointed out during night time by a few rocket release experiments (Rosenberg and Justus, 1966). The Garchy wind measurements systematically confirm this result. Moreover, they show that such motions exist in daytime too.
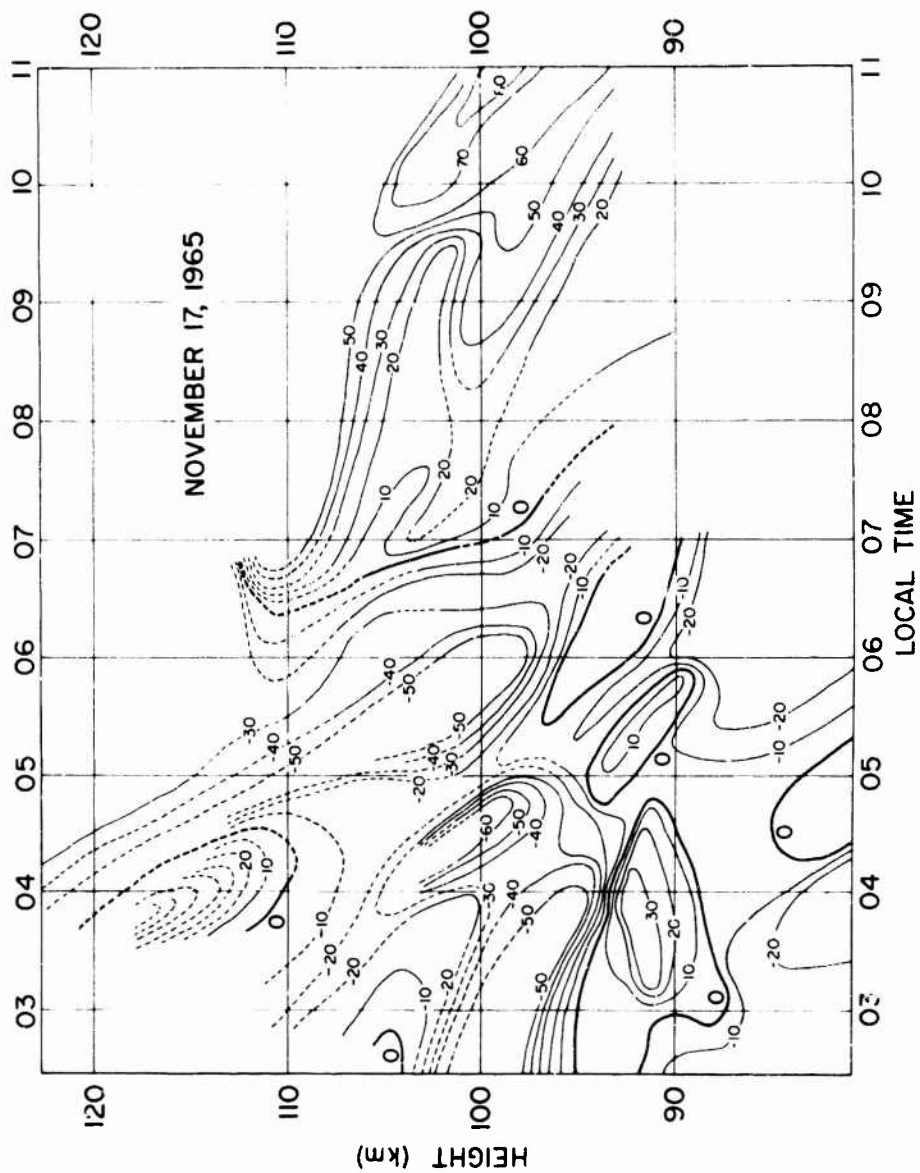
Figure 2. East-West Component of Wind Velocity (in m/sec) as a Function of Height and Time. Positive values are eastward winds, negative ones are westwards

90 – 100 km
NOVEMBER 16,17 AND 18, 1965
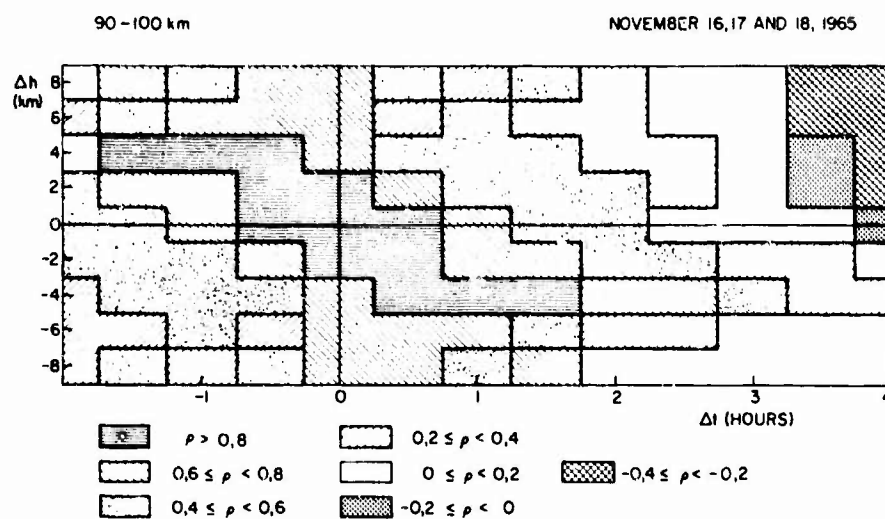
Figure 3. Autocorrelation Function $\rho(\Delta t, \Delta h)$ of the Zonal Wind V (z, t)
as a Function of Lag Time Δt, and Height Difference Δh
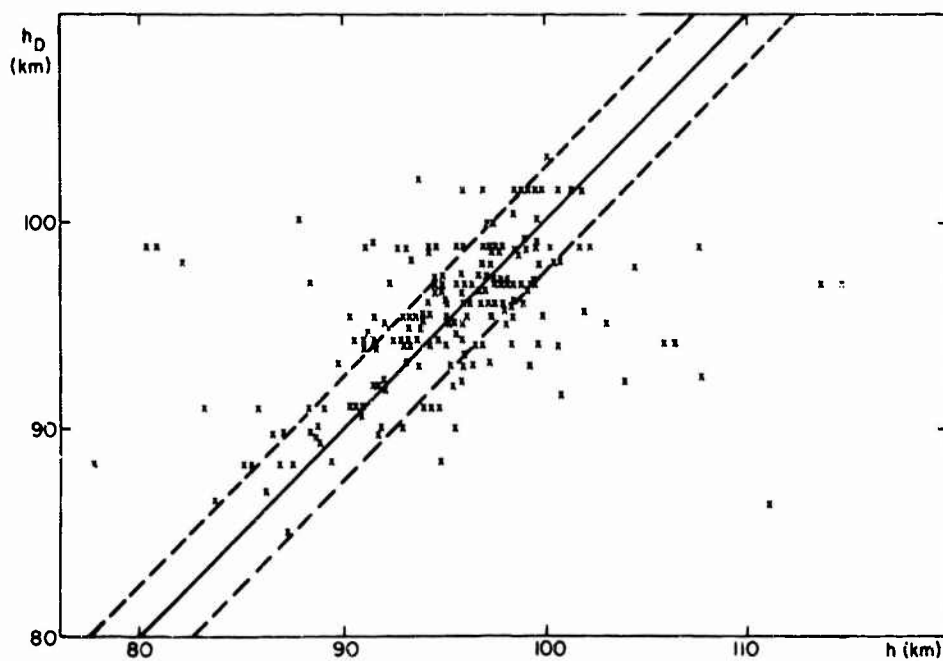
Figure 4. Comparison Between the Height h_D Deduced From the Exponential Decay of Underdense Echoes, and the Height h Measured by the
Garchy Radar (December 12 and 13, 1965)

Strong tidal winds (diurnal and semidiurnal), are also observed. The diurnal has an irregular behavior as a function of height and time. The observation of several parameters requiring a high accuracy on height measurements has been undertaken:

density measurements at ionospheric levels by the echo-decay method: strong correlations were found in the first observations between the height measured and the height deduced from the exponential decay of underdense echoes (Figure 4);

comparison of wind profiles, with the altitude of occurrence of E_s layers, as observed by an ionosonde in the same region.

EDITORS NOTE:

For a more complete description of the system, the reader is referred to Dr. Revah's article in the I. E. E. E.

Five of the slides used by Dr. Revah in his presentation are the transmitting site, Figure 5, at Garchy, the receiving site, Figure 6, at Sens Beaujeu, and the film recordings of trails, Figures 7, 8 and 9.

Figure 5. Transmitting Site at Garchy, France

Figure 6. Receiving Site at Sens Beaujeu, France

Figure 7. Sample of Trail Display

Figure 8. Sample of Trail Display
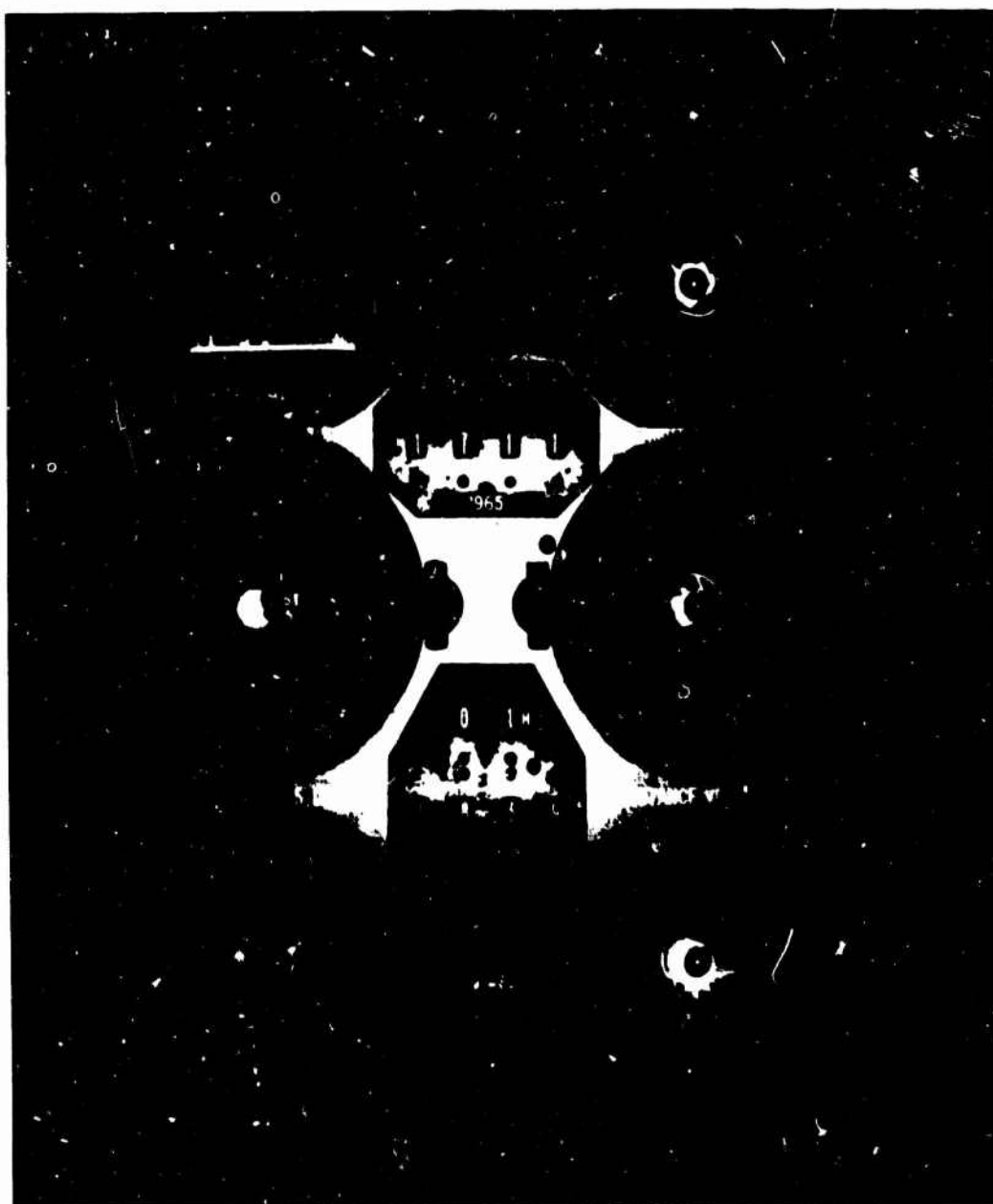
Figure 9. Sample of Trail Display

References

Spizzichino, A. Delcourt, J., Giraud, A., Revah, I. (1965) A new type of continuous wave radar for the observation of meteor trails Proc. I.E.E.E. 53 (8): 1084-1086.

Revah, I., Spizzichino, A. (1966) Resultats de mesure continue des vents ionospheriques a partir de l'observation des trainees meteoriques. C. R. Acad. Sc. Paris, t. 262, Serie B, 378-381 (31.01.66).

Spizzichino, A. (1967) Mesures de vents ionospheriques au moyen d'un radar meteorique, Space Research VII 1: 73.

Rosenberg, N. W., Justus, C. G. (1966) Space and time correlations of ionospheric winds Radio Science 1 No. 2.

VII. The Sheffield University System*

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University of Sheffield
Sheffield, England

A coherent-pulse radar system has been developed at Sheffield to measure the Doppler shift of meteor echoes caused by the bodily motion of meteor trains. Some 70,000 individual echoes recorded in 18 months have been analysed so far to obtain the regular prevailing and periodic tidal components of the upper atmospheric wind system, and the seasonal variations in these components have been established (see paper by Müller on "Atmospheric tides in the meteor zone" on page 319). The Doppler technique employed in the present investigation has also been used to resolve wind velocity gradients.

The coherent-pulse radar system was designed to operate at frequencies near 25 MHz using a crystal controlled transmitter with a peak power output of 20 kW. The receiver is also crystal controlled and a reference signal is derived from the two oscillators in order to switch two phase sensitive detectors in quadrature (see Figure 1). Five-inch double-beam tubes are used to monitor the detector output signals with a time base of approximately 0.5 sec. The magnitude and sense of a drift are thus obtained from traces 1 and 2 and the echo amplitude versus time is displayed on another 5-inch tube (trace 3); trace 4 on a fourth tube displaying the echo range.

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* (Extended Abstract of paper given on 16th of August, 1966)
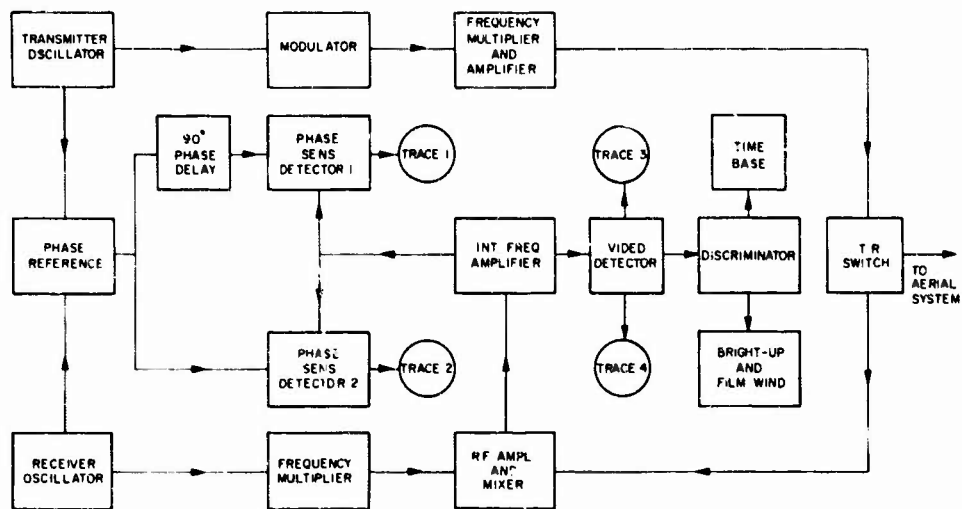
Figure 1. Block Diagram of the Coherent Pulse Radar System
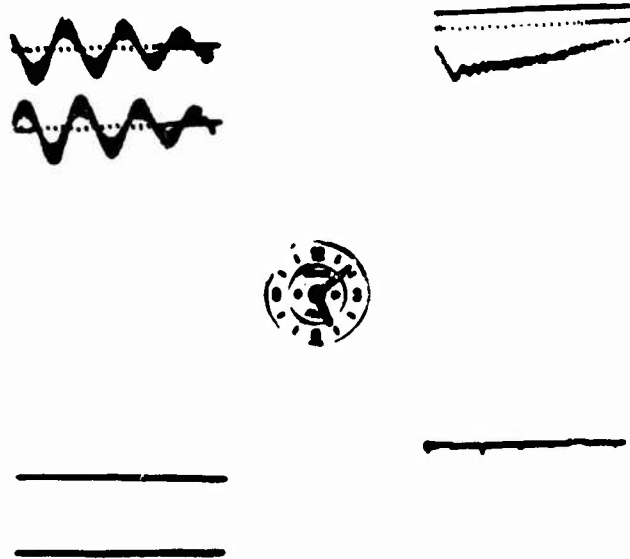
Figure 2. Typical Record

A typical record is shown in Figure 2. The transmitter modulation pattern consists of alternately doubled pulses, 30 μsec long, at a repetition rate of 300 Hz. A discriminator responding to the transmitter modulation pattern is used to re-solve meteor echoes in the presence of strong interfering pulses. Figure 3 shows the principle of the discriminator and Figure 4 is a record of a meteor echo re-corded under very unfavorable conditions. The same aerial (Figure 5) is used for transmitting and receiving, and solid state diodes are employed to switch between the two modes.

The equipment was operated at the Edgemount Research Station of Sheffield University (Lat. 53.43°N Long. 1.35°W). Two fixed 7-element YAGI aerials di-rected NW and SW respectively were used alternatively in order to obtain the aver-age magnitude and direction of upper atmospheric drifts. Usually between 10 and 30 minutes were required to obtain between 20 and 40 meteors for the determina-tion of one orthogonal velocity component.

The radial component Vr of a drift is determined by the rate of phase change indicated on traces 1 and 2 of the display unit. Where more than half a beat cycle is resolved on the records the velocity follows directly from the spacing of the amplitude zeros (Figure 2), otherwise the phase change has to be determined by taking amplitude ratios (Figure 6) at two different times. On occasions the echo height h required for the determination of the horizontal drift component V = Vr/$\cos\theta$ (where θ = $\sin^{-1}$(h/R); R = range) has been estimated from the decay rate of underdense trains (Figure 7) but in general a standard height of 95 km has been adopted. Subsequent analysis is carried out separately for data obtained on each aerial, and only the final results are compounded to obtain the meridional and zonal components of a drift. Figure 8 shows the theoretical error limits (dashed curve) considering the effects of (a) aerial polar diagram, (b) phase errors, (c) height errors, and (d) range errors. The solid curve refers to the spread of data actually observed. The larger increase of the error limits appears to be due to the presence of a vertical wind shear, so that meteors recorded at different heights will exhibit different drift velocities.

Figure 9 shows a typical set of wind data obtained during a 24-hour run. The data have been used in a curve-fitting analysis where the constant and harmonic terms are resolved in the form

$$V(t) = A_o + \sum_{i=1}^{4} \left\{ A_{2i-1} \cos \frac{2\pi i}{24} t + A_{2i} \sin \frac{2\pi i}{24} t \right\} .$$
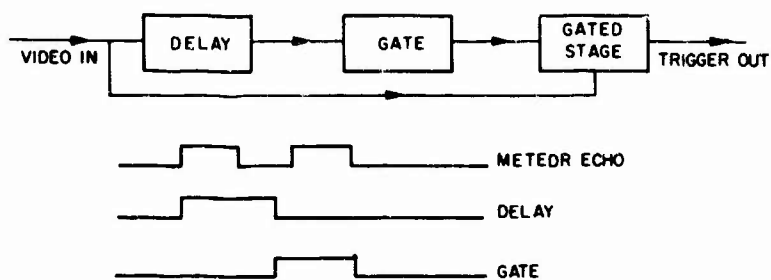
Figure 3. Block Diagram of Discriminator
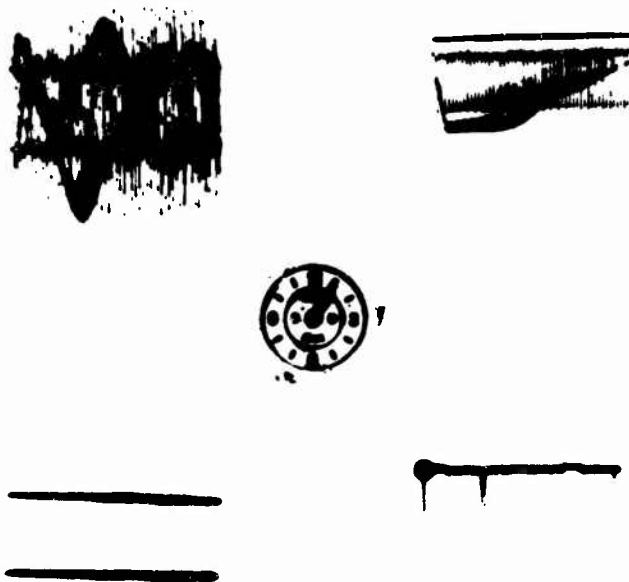
Figure 4. Record of Meteor Echo

Figure 5. Transmitting and Receiving Antennas, Edgemount Research Station, University of Sheffield
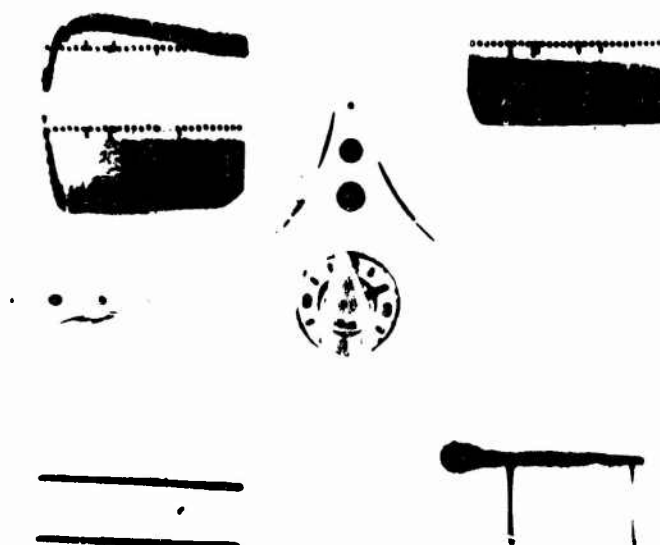
Figure 6. Record of Meteor Echo
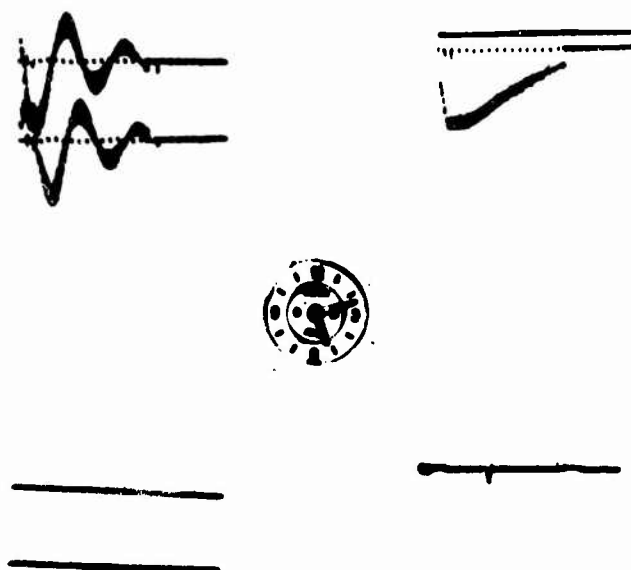
Figure 7. Record of Meteor Echo

The individual cos and sin terms are then compounded to give an equation of the form

$$V(t) = V_o + \sum_{i=1}^{4} V_i \sin\left(\frac{2\pi i}{24} t - \phi_i\right).$$

We thus derive the NE and SE components of the wind which we compound to obtain the meridional (NS) and zonal (EW) components.

The Sheffield University system is being extended at present. The frequency of operation will be near 36 MHz, and a 200 kW power amplifier is to be used in connection with three 16-element twin YAGI aerials. Phase measurements will be included in order to obtain the azimuth and height of a reflection point and with the aid of two remote receiving stations at a distance of 4 and 20 km respectively, the vertical fine structure of the wind system is to be studied. It is anticipated that the new system will be fully operational by Spring 1967.
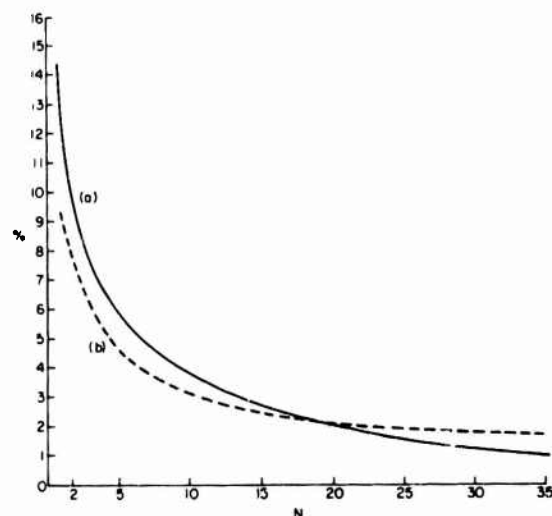
Figure 3. Error in Velocity Measurement (95% confidence limits) as a Function of the Number N of Meteor Echoes Used in Computing the Average Horizontal Wind Velocity. Curve a) is the result of the analysis of 40 meteors recorded within 9.5 minutes, and curve b) is the theoretical limit without considering vertical wind gradient. The values for curve a) are based on an average horizontal velocity of 48.68 msec^{-1}
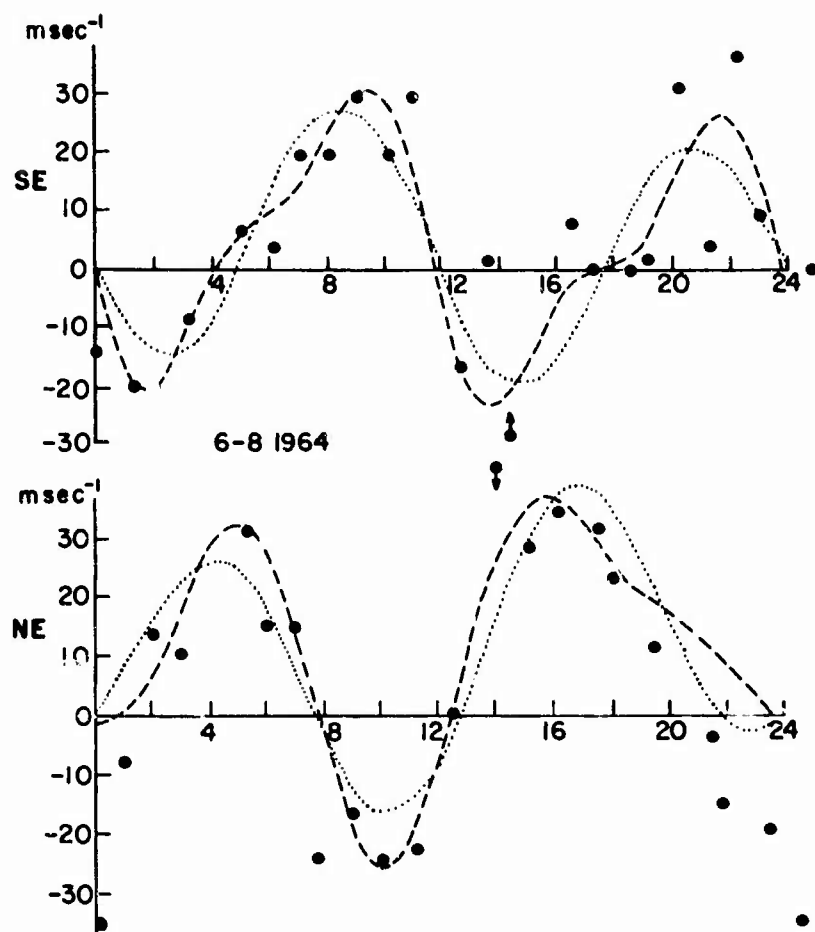
Figure 9. Experimental Wind Velocities
and Computer Fitted Curves for 3(....)
and 5(----) Harmonic Terms

Session 2

Height and Wind Accuracy

Chairman

Dr. Arnold A. Barnes, Jr.

I. Chairman's Remarks

DR. BARNES. Professor Reginald Newell from the Meteorology Department at Massachusetts Institute of Technology was to have chaired this session on the determination of winds and height and the accuracy of these parameters, but unfortunately Reggie is ill and sends his regrets that he can't be with us today. 1 picked Professor Newell because he is particularly fitted to chair this session. He took his undergraduate work in England and then came to the United States and got his doctorate at the Massachusetts Institute of Technology in the field of Radar Meteorology.

Since then, he has worked in the field of general circulation of atmosphere. We worked together in the field of general circulation of the stratosphere. Since then, Reggie has moved up to the mesosphere and has published a few papers using the rocket wind data in order to compute momentum transport. He has also been teaching a graduate course at the Massachusetts Institute of Technology on the physics of the high atmosphere.

Professor Victor Starr, Professor Newell, and 1 have had a number of discussions concerning the use of radar meteor trail winds and densities in determining the general circulation of the mesosphere. The first question that we asked ourselves was whether there is enough information available to perform studies of the general circulation, and the answer is, emphatically, yes! There are the noctilucent cloud winds, rocket measurements, visual meteor trail winds, and radio meteor trail information available.

Figure 1 shows the stations over the world, which, in my estimation, are capable of obtaining atmospheric wind information. First, there is the set at

EXISTING RADAR METEOR TRAIL WIND SETS 1966
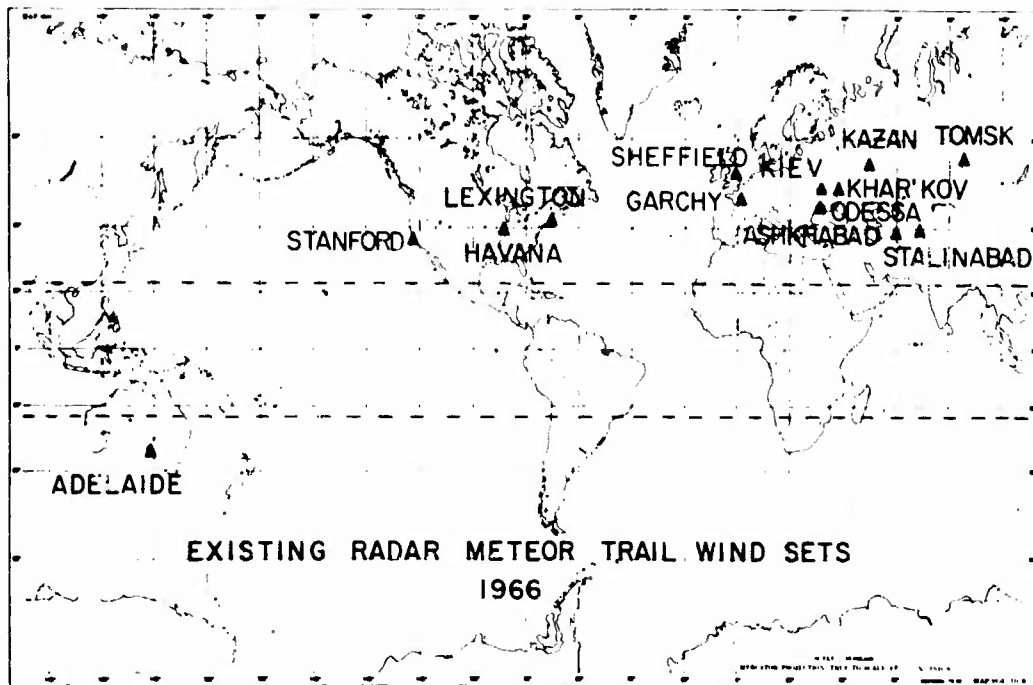
Figure 1.

Adelaide, Australia operated by the University of Adelaide. Dr. Ellyett has said
that on Friday he will tell us what they are doing at Newcastle, Australia and what
they are planning to do. At Stanford, California, Prof. Peterson is taking obser-
vations for us under contract. There is the set at Havana, Illinois, operated by
the Smithsonian Astrophysical Observatory. The AFCRL set is listed at Lexington,
because it is actually just across the town line in Lexington. The work in England
is now being carried on at the University of Sheffield. The French set at Garchy
is being operated by CNET. According to the literature, the Russians are opera-
ting sets at Stalinabad, Ashkhabad, Odessa, Kharkov, Kiev, Kazan, and Tomsk.
That is seven, and I understand three other sets have been constructed and are in
operation in Russia. But you may have noticed, from reading the literature, there
is not very much new data coming out. The Russians are investigating the winds
for the support of aerospace vehicles, but most of the papers that they have pub-
lished have been more or less a rehash of the work that was done by Greenhow and
others.

There are three other sets that are going into operation. There is one up at
Fairbanks, Alaska. NSF is funding the University of Alaska to put a set up there.

There is another site proposed at White Sands, New Mexico. The Army has put funds aside for this, and I understand that they are going ahead with a set which will operate at 32.6 MHz.

The Russians are going to move a set down into equatorial regions. The information appeared in Pravda that they are constructing a set to move down into one of the countries along the eastern coast of Africa. No date was given.

MR. MÜLLER. Perhaps I could just mention that we are planning joint exercises to do pulse radar down at Ghana. This is two degrees north.

DR. BARNES. Will you run into the electrojet down there?

MR. MÜLLER. Yes, this is a problem.

PROFESSOR PETERSON. Well, it certainly shows up on 30 MHz in the daytime because we ran into it in Peru during the IGY; and we had this sort of continuous backscattering on 30 MHz on east to west.

DR. BARNES. This is perhaps one of the major things that should be considered before moving a set into the equatorial regions.

The dashed lines on Figure 1 encompass the tropical region. There are no sets in this region, and if you look at the geometry of a vehicle coming back to the earth from the moon or beyond, you will find that it will probably make its initial penetration into the upper atmosphere in the equatorial region. For this reason, I think, we must obtain observations in the equatorial region in order to provide support for these vehicles. Are there any more questions with respect to Figure 1?

MR. MÜLLER. There is a possibility that another set can be put up, I think, near Kingston, Jamaica. Raymond Bright spoke to me about this, but they haven't anybody who wants to spend three years up there.

PROFESSOR PETERSON. We have been approached at Stanford by several people from Brazil to supply them with equipment. Our problem has been to find an engineer with the know-how who was willing to go in there.

MR. NOWAK. They have an equatorial rocket launching site at Belem. They could support an operation.

DR. MILLMAN. You might bear in mind the HARP Project at Barbados, from which they are getting very good wind determinations from the gun launch vehicles. That would be a possible site if one wanted to install equatorial radar. The conditions for installing equipment there are good, and there already is upper air work going on at Barbados.

DR. BARNES. To return to the point I originally wanted to make, there are a number of stations that are taking observations all over the world. There are more stations here than were originally used by meteorologists when they first successfully attacked the problem of the general circulation of the troposphere and stratosphere, and with this number of stations I think that the meteorologist can begin to draw some correct conclusions about the meteor trail region.

Radiosondes reach to 30 km or above, the meteorological rocket networks provides information up to 60 or 70 km, and meteor trails supply data from 80 to about 110 km. If an effort were made to obtain observations by all three methods at all stations during one period, then the meteorologists could learn quite a bit about the atmosphere as a whole from the surface to 110 km.

DR. NILSSON. For historical purposes, the Antarctic equipment is right down at the bottom right hand corner of Figure 1.

DR. BARNES. Yes, but it is no longer operating there. Maybe you would like to take it back down there and make some observations. The meteorologists would love it.

To get all the sets working at the same time would be a problem. But, I think the point should be emphasized that we do have these sets, and we can get enough observations to attack the problems of the general circulations of the lower thermosphere.

Now, if the quantity of the data is sufficient, what about the quality? Can average winds based on fractions of a second be taken as representative of the large-scale synoptic and diurnal motions, or do gravity waves and turbulence mask these motions? How good are the wind and height determinations as obtained by the various systems? It is to this last problem that we now address ourselves. Our first speaker will be Dr. Robert Roper who is at the Georgia Institute of Technology at Atlanta, Georgia, and he is going to talk on the Adelaide system, where he did quite a bit of work.

II. Height and Wind Accuracy, University of Adelaide System

Dr. R. G. Roper
Georgia Institute of Technology
Atlanta, Georgia

Abstract

The University of Adelaide Meteor Wind System combines both cw and pulse techniques and an "all sky" antenna system to determine winds in the meteor region. The pulse radar is used only to determine the range of the meteor trail reflection point from the TR system. The accuracy of range determination is better than ± 2 km. The direction of arrival of the echo is determined by a comparison of the cw signals as received by an array of five suitably spaced antennas. Error in the determination of angle of arrival is less than 1°. The error in the height calculated from the echo range and zenith angle is thus of order ±2 km.

This accuracy of echo height determination is based on a theoretically "perfect" trail, one which is drifting under the influence of a constant wind. If a wind gradient is present, trail rotation will occur and, although the line of sight distance (range) will not change appreciably, the reflection point can move several kilometers along the trail in less than a second. Such echoes are easily recognized, since they are characterized by a time dependent Doppler shift. However, since most drift determinations are made over a time interval of less than 0.3 seconds, change in echo height due to trail rotation is usually less than a few hundred meters.

Height differentials as measured on a single trail by the spaced receiving station technique are considerably more accurate than the differences in echo height determined by comparing the absolute heights of more than one trail. Height differences on an individual trail can be determined to an accuracy of better than 30 meters per kilometer.

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The absolute accuracy of the line of sight drift measurement depends on the rate of drift of the trail, but is generally better than ten percent of the drift speed, i.e., ± 3 meters per second for the average 30 m/sec line of sight drift. The error associated with the wind determined by the technique of data reduction used at Adelaide prior to 1961 was influenced not only by inaccuracies in the echo height and direction of echo arrival measurements but also by the low echo rate. Higher powered transmitters, which have greatly increased the echo rate, have recently been installed. Since 1961, an improved data reduction technique has been in use, in which the individual line-of-sight measurements are matched against a wind model, the coefficients of the polynomial variations allowed by the model being calculated by a least squares fitting of the model to the data. The results of this analysis have shown that an individual echo height accuracy of ± 2 km is quite adequate to determine the height variation of the mean wind (prevailing plus tidal components). The greater height difference accuracy of the multi-station technique is required for any meaningful determination of turbulent wind shears.

Before discussing the accuracy of echo heights as measured by the Adelaide equipment, I should, perhaps, emphasize one or two points which have to some extent been glossed over. I am sure that all the people who are working in the radio meteor field realize that the method does have limitations. This may not altogether have been conveyed well to the others present. For example, in talking about just how good one's ranging and drift determinations are, the target is always assumed to be a nice straight meteor trail. Very often you do get echoes in which this is the case, and you can realize almost the full capability of the system. However, you just as often as not get the phenomenon that was pointed out yesterday (of trail rotation due to wind gradient) and under these circumstances you will find, as Mr. Müller has shown, that you get motion of the reflection point along the trail.

In general, with the wind gradients that are normally encountered in this region of the atmosphere, you will not get a very large shift in height. Since most wind measurements are probably taken in the first fifth of a second or so, then, although the reflection point can be moving along the trail at speeds of the order of several kilometers per second, one doesn't get more than a few hundred meters change in height over the interval of time it takes to measure the Doppler.

In addition to this change in height, reflection point motion introduces errors into angle of echo arrival measurements, which rely on comparisons between signal phases at different antennas. It is difficult to determine accurate direction cosines of the reflection point by comparing the phases of signals at each antenna, when the Doppler beat frequency is changing with time; so it is not always possible to realize the potential accuracy of a system when one comes across this type of echo.

As I have said, most of the time these limitations aren't very serious, but it should be borne in mind that such limitations do exist.
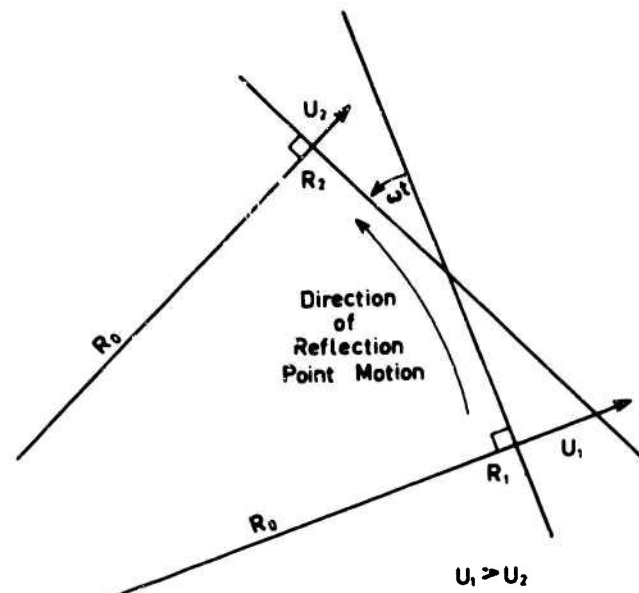
Figure 1.
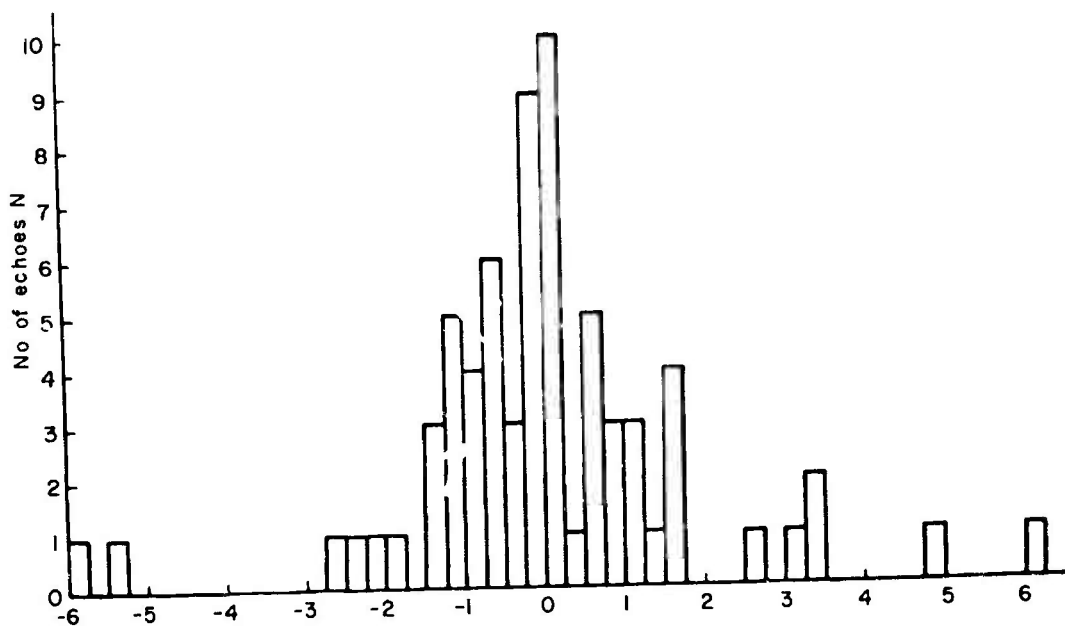
Figure 2. Motion of Reflection Point Along Trail, (km/sec) December 1960

Figure 1 illustrates this point, and from the diagram one can compute the rate of motion along the trail. In 1960 and 1961 we carried out at Adelaide a three-station survey in which we measured the differential drift velocities along each trail. We compared these gradients with the change in Doppler beat type of shear determination, and found quite good agreement. In other words, it is quite practicable to measure shears with a single station radar using the method which Mr. Müller demonstrated yesterday.

Figure 2 is a histogram of the reflection point motion speeds determined from 72 echoes in December 1960. This histogram is derived from shears determined from single station long-duration echoes; but as I have said, these did correlate well with the actual shears measured from spaced stations.

As you can see, motions along the trail of the order of a kilometer a second are really quite common, and if one is interested in a high order of accuracy in the determination of position on the meteor trail, measurements must be taken within a short interval of time or with sufficient angular resolution to resolve differences of the order of a few hundred meters of trail length.

Figure 3 shows some typical echoes taken from the equipment at Adelaide. Three types have been chosen. The trace at the top shows a definite change in the Doppler beat with time. It also shows some amplitude fluctuations, which are a product of the geometry of the reflection rather than variations in ionization along the trail. All three traces in this figure are records of long duration echoes, which can be useful in determining some wind characteristics. The first echo can be explained simply by motion of the reflection point along the trail into areas of differing wind velocity. In the second case, one has apparently two reflection points moving with slightly different drift velocity. This trace was recorded some time after trail formation, sufficient time having elapsed for the trail to have been twisted by winds so that two segments of the trail were aspect sensitive. These segments are moving with a slightly different drift velocity, giving rise to the additional beat component characterized by the envelope of the Doppler. This type of echo occurs only some time after the formation of a long duration trail.

When looking at long duration trails, one quite often detects an echo which does not show the Fresnel buildup. A trail can assume aspect sensitivity by being blown around by the winds, and this can sometimes happen many seconds after the influx of the meteor.

Trace number 2 in Figure 3 is probably useful since the envelope, which corresponds to a relative drift velocity between the two reflecting centers of only a few meters a second can be disregarded, and the frequency within the envelope taken as the rate of drift.

All that can be said about the bottom trace of Figure 3 is that the situation is chaotic, and this type of echo would be neglected for routine analysis.
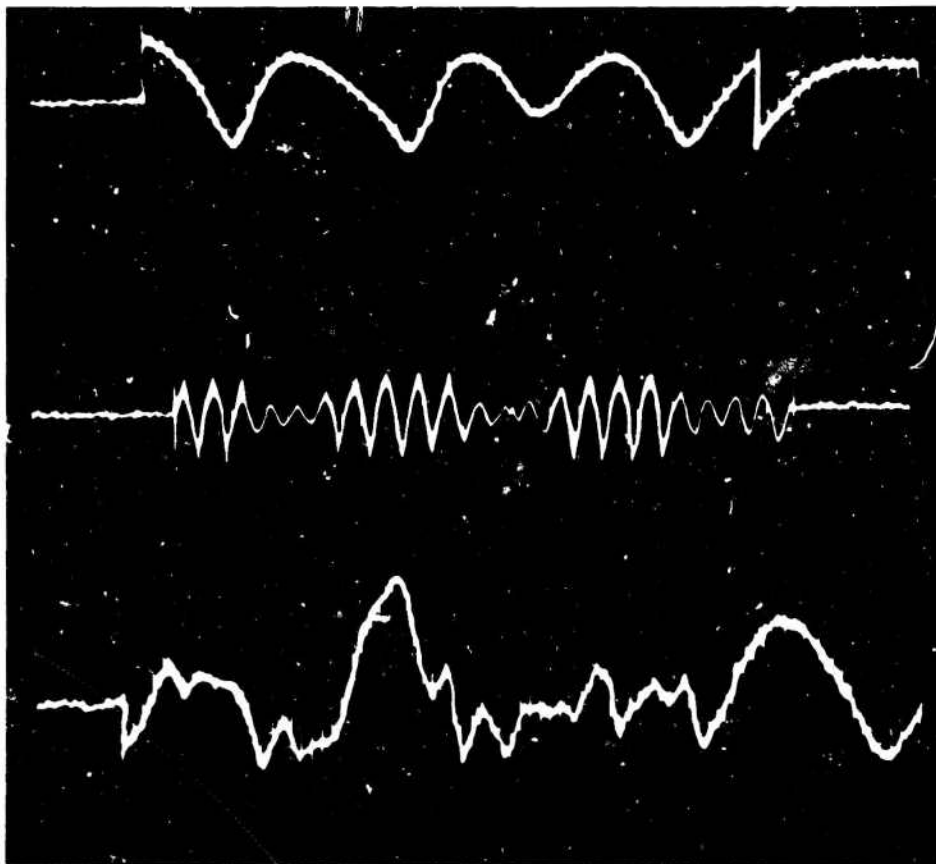
Figure 3.

DR. ELFORD. Bob, before we leave this slide, which could perhaps give a wrong impression; these are typical of long duration trails, and not of the majority of cases.

DR. ROPER. That is so, and it is something I should have given greater emphasis to. I did mention at the start that it is on long duration echoes that these effects are observed. Most echoes obtained with a reasonably sensitive system are from underdense (short duration) trails, which do not show most of these effects, since they are detected only if they are aspect sensitive during the formation of the trail. One has no opportunity to see them at later times.

PROFESSOR PETERSON. In Figure 2 you show December measurements of this motion along the trail, and I was wondering if there was any idea of what percentage of meteors showed reflection point motion?

DR. ROPER. This was a sample of 72 out of approximately 120 echoes. You must bear in mind here that the 1961 survey was working at about a magnitude of

plus six, just on the borderline between long and short duration trails, due to low
transmitter power. We have since increased the power significantly (cw radar,
from 300 watts to 1.5 kW; pulse radar, from 5 kW to 50 kW). For short duration
echoes, you can neglect the effect of reflection point motion. *

In Figure 4, I have plotted the echo rate against height and time for the month
of September 1961, the total of nine days data being lumped into one "typical" day.
The numbers in each 2 km by one hour box represent the incidence of meteors de-
tected by the system. There were 926 meteors recorded in this nine-day interval.
As can be seen from the table, there is a tendency for clustering to occur. There
is a large number of echoes in the period from six to eleven o'clock in the morning,
and there are various places in which quite large holes appear, particularly in the
late afternoon. Large sets of zeroes appear in the upper levels and also at the
lower levels between 13 and 21 hours, the period of lowest rate for the day. The
diurnal rate variation is about six to one, and can in fact be much worse than that,
going up to about ten to one. We are using, I might add, an all-sky system. We
look for echoes within a zenith angle of 60°. As smaller and smaller beam widths
are used, as was pointed out yesterday, the diurnal rate variation becomes much
larger.

I think this is the appropriate time to say that in deducing heights, our main
limitation lies, in general, in film reading. The angle of arrival of each echo is
determined from the relative phases of the Doppler beat at spaced receiving anten-
nas, as Dr. Elford showed when he described the equipment yesterday. We can do
this to a reasonable order of accuracy--about one degree in both azimuth and
elevation.

To determine the range, we read a conventional intensity-modulated A-scan,
which presents the echo as a line between range markers. This can be done to an
accuracy of about ± 2 km. The actual equipment parameters enable a much more
accurate range measurement (± 200 meters), and inclusion of a clock and digital
readout is contemplated, but for the present the photographic presentation limits
the range resolution. All the data presented here has been smoothed by up to
± 2 km variation in height determination.

Figure 5 gives the echo rate for the whole year 1961, plotted as the number of
echoes against height. Whereas we at Adelaide have always felt that it is particu-
larly important to measure height and determine height shears, if height is

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*(Note added in proof: Rather, you have to neglect it, because there is insuffi-
cient time available to sensibly measure any change with time of the Doppler beat
frequency. However, because of the short duration over which the Doppler frequen-
cy is determined (usually less than 0.2 sec), the reflection point will have moved
only a couple of hundred meters along the trail anyway.)

RESULTS FOR SEPTEMBER, 1961. USSL SHORT PERIOD SPECTRUM. RUN 16/17

ECHO RATE AS A FUNCTION OF TIME AND HEIGHT.

Figure 4.

Figure 5.

neglected one still gets a fairly good wind determination at the height of the peak in the height distribution. The height at which the maximum number of echoes is obtained depends on the radar frequency. As the radio frequency is increased, the peak of the distribution occurs at lower heights, where the trails will have durations long enough to be recorded. The increase of the ambipolar diffusion coefficient with height results in shorter duration echoes at greater heights.

DR. BARNES. It also turns out that the number of returns is influenced by the direction at which your set is pointed, because you will pick up more echoes at certain times of the day, and you also find that this mean height changes during the day.

DR. ROPER. Well, with our all-sky system, it doesn't.

DR. BARNES. That is right, but most people still have directional beams. We very definitely found this in our data.

PROFESSOR PETERSON. What sort of height variations?

DR. BARNES. It seemed to me it was of the order of a scale height. I knew it was enough so that we could not try to use the mean winds averaged without regard to height to look at the diurnal variations, since the mean height moves up and down about one scale height and could change the wind direction by 180°. In order to get the best results one should go to an all-sky type of system where one can measure azimuth as well as height.

PROFESSOR PETERSON. Isn't it true that even with the all-sky system, the direction from which the meteors come will vary during the different times of the day and the mean wind velocity will vary with the direction also?

DR. BARNES. It does depend upon your beam width.

DR. ROPER. To continue: in presenting particular data here today, I have used September 1961, because it was an interesting month with considerable wind structure with height. I will talk about this later on.

Returning to Figure 5, the dots represent the sum total of echoes for the year 1961, and the triangles represent the echoes for September. The two sets of results were roughly normalized by multiplying each of the September values by ten (9000 echoes for the year, 926 for September). As you can see, they fall close to the overall curve, except for a couple of points around 95 km. These echoes could have been associated with the type of phenomena that Professor Peterson has just mentioned, and in particular, possibly a meteor shower.

Meteor showers are important from the point of view of the astronomical side of the experiment, and are a complicating factor in terms of determining diurnal wind, for example, because a meteor shower will exhibit aspect sensitivity only for a short period of each day. There will be a high echo rate during that period of the day, but for the rest of the day the rate will fall back to normal. This may be useful, of course, if one wishes to sample a large number of echoes in a small amount of time. Another factor with showers is the distribution of the meteor mass and the importance of the antenna system parameters.

PROFESSOR PETERSON. How do these figures compare with yesterday's estimates of 500 targets per hour?

DR. ROPER. In the 1961 survey at Adelaide the rate was something of the order of three or four usable echoes per hour. As I have said, the September data of 900 odd echoes was collected over nine days, and these have been lumped together to give winds for a "typical" day. I will show later that the procedure is valid by presenting some wind spectra.

PROFESSOR PETERSON. Well, did you just ignore the rest of them, or were you unable to find them?

DR. ROPER. As I said earlier, we had an unfortunately low power level and this, together with the use of cw, does not give a high rate. We could have possibly read more echoes if we had had a better radar, but we were getting only 5 kW out of the pulse transmitter at that time. This cut our rate down to the point where we were working on the borderline between underdense and overdense trails.

Figure 6 shows two typical echoes as recorded by the routine wind measuring display. In the first (echo number 061229), decay is reasonably exponential in the first instance. It is possible that another echo occurred toward the end of this recording interval, where some Fresnel modulation can be seen. The envelope of
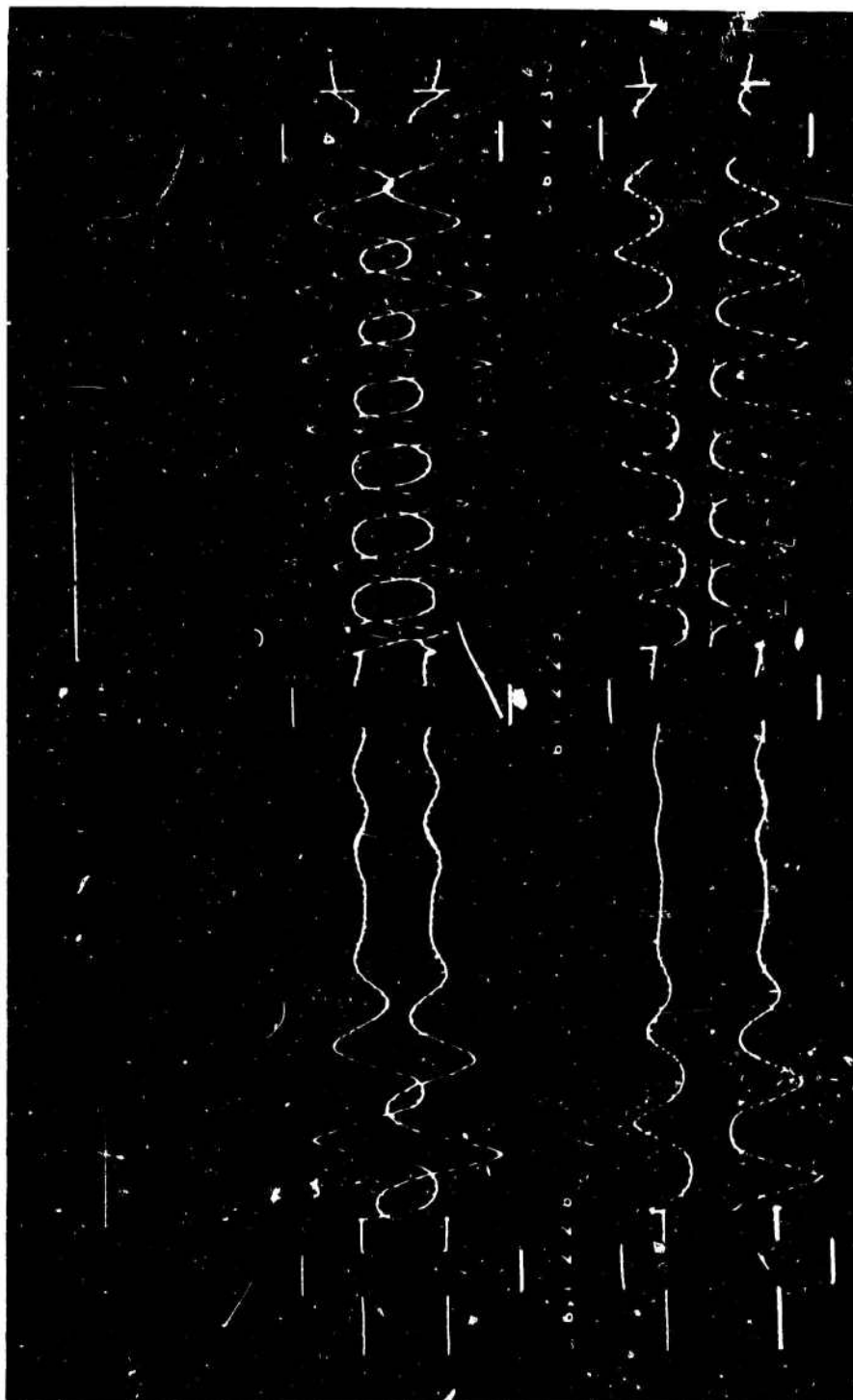
Figure 6.

the initial Doppler beat is approximately exponential, so this could be taken as a short duration echo. Echo number 061230 is obviously a long duration echo. The range markers (top ten traces) are 20 km apart; the range can be read from this scan to about plus or minus two kilometers.

Figure 7 shows the outputs from the three separate recording displays used in 1961 (the poor signal-to-noise ratio is due to the low power then being used). The top three traces (with clock) show the buildup of the Fresnel pattern at each of the three receiving stations; the longer wavelength beat at the end of each trace is the Doppler caused by trail drift due to wind. From the Fresnel pattern, the time of arrival of the meteor at the specular reflection point corresponding to each of the three stations can be determined.

The four traces at the bottom left of Figure 7 depict the line-of-sight drift at each of the three receiving stations operating during the 1961 survey. As I mentioned before, we can measure the height differences between reflection points to a reasonably high degree of accuracy (± 30 meters). However, in the '61 survey, because of the small station separation, the maximum height difference measured directly was two kilometers. Even so, one can see here evidence of quite a reasonable shear. The top trace has the fastest Doppler, the bottom an intermediate Doppler, and the middle traces the slowest Doppler. The change in wind speed indicated by these Dopplers is a factor of two to one over a height difference of less than 2 km.

In the bottom right frame of Figure 7, you can see the problem we had with signal to noise in 1961, even with a relatively strong echo, especially when compared with Figure 6, which was produced by the present system.

MR. MYERS. Could you please explain what the traces in the bottom left of Figure 7 are?

DR. ROPER. The top trace is the signal which is sent back from the receiving station some 5 km to the north of the main receiving station. The two center traces are mirror images and present the Doppler beat as it was measured at the main station. The bottom trace is the beat measured at the third receiving station 5 km to the east of the main station. The signals are sent back to the main station on FM links, so that all recording is done at the one site.

Each of the frames that make up Figure 7 refers to the same echo. The top frame (with clock) was recorded at six times the film speed of the other two displays, in order to resolve the higher frequency components of the Fresnel pattern. The phase spikes which are evident on all traces (except the radar scan) occur at 20-msec intervals.

Figure 8 shows a few more echoes. Once again, long-duration echoes have been chosen as illustrations (they last long enough for one to "see" what is happening). A considerable shear exists between the main station echo point, as

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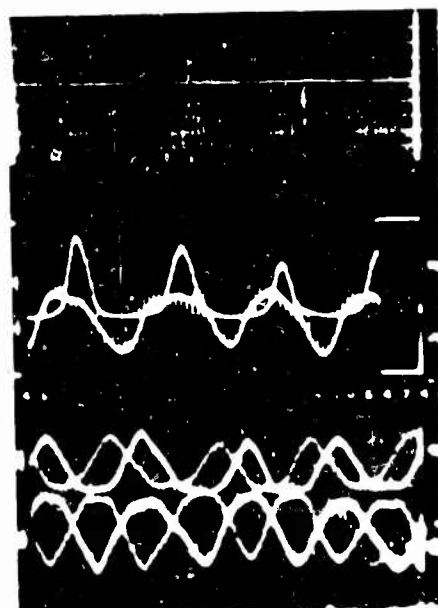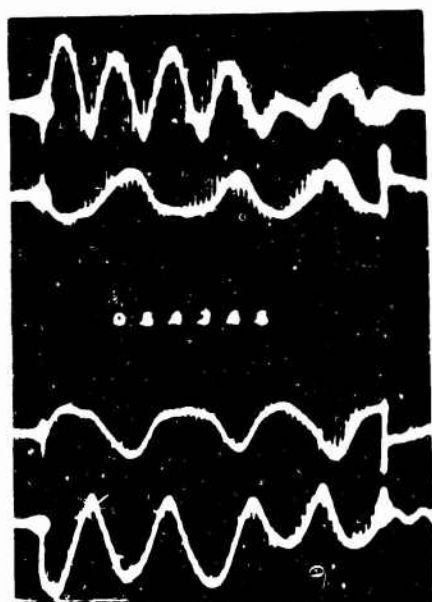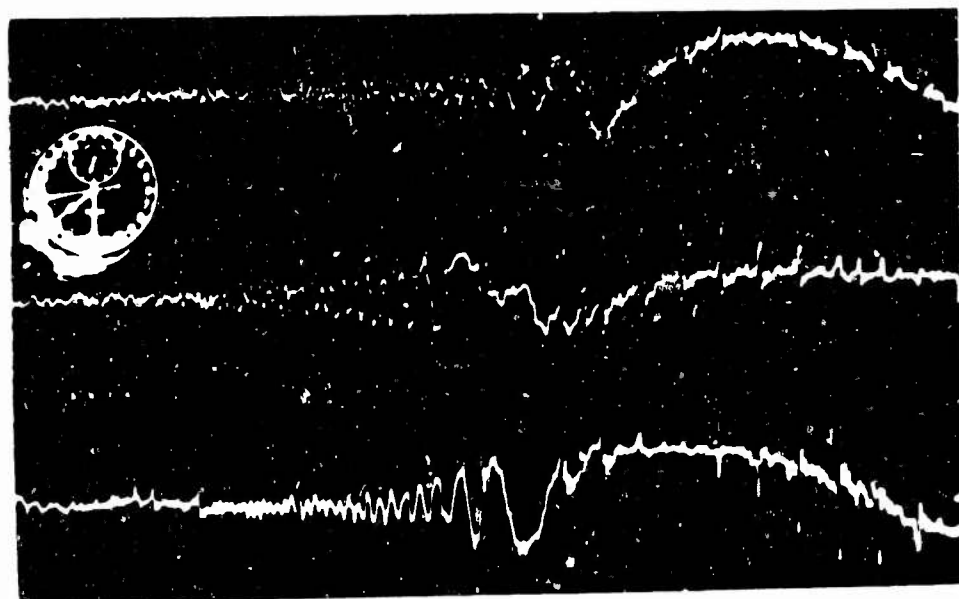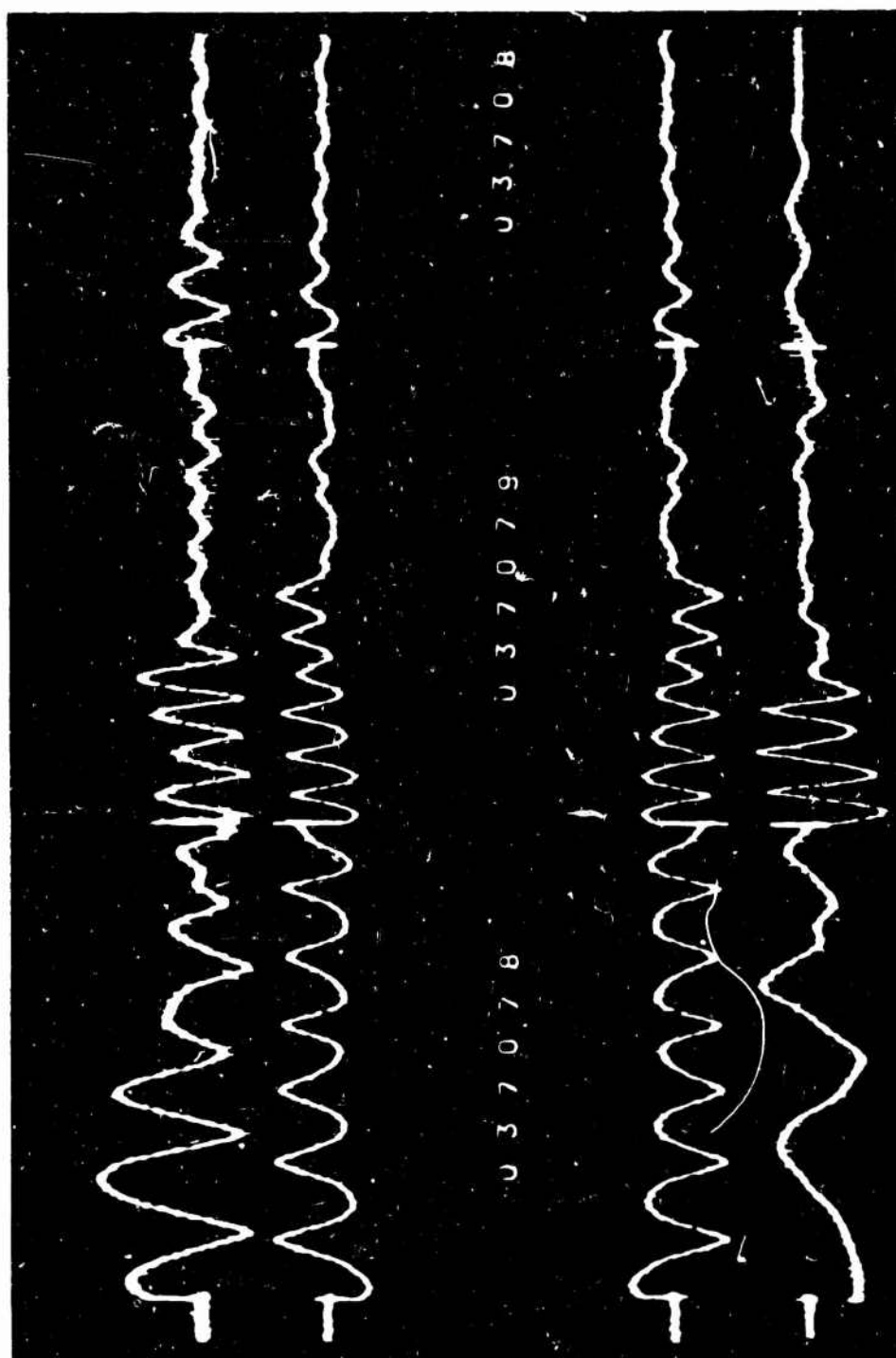
Figure 7.

Figure 8.

RESULTS FOR SEPTEMBER, 1961.   GSFC   SHORT PERIOD SPECTRUM.   RUN 16/12

EAST-WEST COMPONENTS OF THE MEAN WIND, AMPLITUDE AND PHASE,

AS DETERMINED FOR THE HEIGHT RANGE   75 KM TO 105 KM.

HEIGHT	MEAN	ERROR	24.0 HOUR COMPONENT				12.0 HOUR COMPONENT				8.0 HOUR COMPONENT			
			AMP	ERROR	PHASE	ERROR	AMP	ERROR	PHASE	ERROR	AMP	ERROR	PHASE	ERROR
105	-68.	46.	69.	42.	21.0	4.2	122.	56.	8.9	0.8	70.	42.	2.2	0.8
103	-55.	29.	17.	40.	0.4	4.1	83.	35.	9.2	0.7	35.	26.	2.3	1.0
101	-41.	17.	35.	19.	6.7	2.7	56.	21.	9.6	0.6	13.	15.	2.5	1.6
99	-24.	10.	55.	11.	7.6	1.0	36.	12.	10.1	0.6	3.	10.	4.0	4.3
97	-16.	6.	62.	8.	8.0	0.6	21.	8.	10.4	0.7	4.	7.	5.5	2.4
95	-6.	5.	59.	7.	8.5	0.5	9.	7.	10.3	1.5	2.	4.	5.2	4.5
93	3.	5.	48.	6.	9.1	0.5	6.	6.	7.1	2.1	5.	6.	2.4	1.6
91	10.	4.	35.	5.	10.3	0.7	15.	6.	6.3	0.7	13.	6.	2.3	0.6
89	14.	5.	27.	6.	12.5	1.1	24.	7.	6.3	0.5	22.	6.	2.3	0.4
87	15.	6.	29.	9.	14.9	1.0	29.	8.	6.4	0.5	29.	7.	2.4	0.3
85	13.	7.	35.	11.	16.3	0.8	28.	9.	6.6	0.6	32.	8.	2.6	0.3
83	6.	9.	37.	14.	16.9	0.9	21.	11.	6.9	1.1	32.	10.	2.8	0.4
81	-1.	15.	30.	24.	16.9	1.7	8.	16.	8.9	4.6	30.	16.	3.4	0.6
79	-15.	26.	13.	35.	14.4	9.6	29.	31.	11.9	1.8	37.	28.	4.3	0.7
77	-33.	42.	45.	56.	7.9	5.9	71.	47.	0.2	1.2	64.	43.	5.0	0.7
75	-47.	64.	44.	94.	7.2	3.1	130.	74.	0.4	1.1	111.	63.	5.3	0.7

Figure 9.

represented by the two center traces, and the third station at the bottom. These shears in general are transient phenomena, rather than being true shears in the mean wind. I will say a little more about this later.

Figure 9 gives some idea of the magnitude of the west-to-east component, as measured in September 1961, and the errors associated with their determination. As you can see, the errors are considerable as one approaches the extremes of the height range, and this is to be expected when due consideration is given to the rate profile of Figure 4. We would ignore those values above 100 km and those below 80 km. The mean (or prevailing) wind here has a quadratic variation with height. There are two wind zeros in the prevailing zonal component--one at 94, the other at 82 km. In the equinoctial months, the 24-hour component has a reasonably high amplitude, as you can see here, --62 m/sec at 97 km, 27 m/sec at 89 km, and up to 37 m/sec at 83 km. The error values are calculated from the least squares matching of the data to the model. At 80 km the error is approximately 24 m/sec. In the 83-to 99-km height range, where we recorded most of the echoes, the errors run between 5 and 10 m/sec. Above 100 km, errors increase rapidly.

PROFESSOR PETERSON. Are the errors one or two sigma?

DR. ROPER. They are one sigma, or, more correctly, one rms deviation.

For the 12-hour component, notice the low amplitude at the mean echo height of 93 km. This component is not significant at this level, in that its amplitude is only equal to the error associated with its determination. At the same height, the diurnal wind amplitude is 48 m/sec, with a 6-m/sec error.

We also measure, or rather simultaneously extract, an 8-hour component. The 70 m/sec 8-hour amplitude at 105 km is obviously not real, and the 40 m/sec error associated with it is an indication of its unreliability. At lower heights the amplitudes are less than the errors, but in the 83-to 89-km height range it does appear that some significance can be associated with this 8-hour component. At 87 km, the amplitude of 29 m/sec has an error of 7 m/sec, so this probably does represent a real 8-hour component over the nine days of observation.

DR. MILLMAN. Pardon me, did you choose that 8-hour component? Did it come out of the data automatically, or did you just pick an 8-hour component and then analyse on that basis.

DR. ROPER. In setting up our model, we allow polynominal variations in height for a prevailing wind, a 24-hour component, a 12-hour component, and an 8-hour component. We then sum through the sequence of harmonics, matching the data against the model, allowing each of the chosen components to have polynominal variations with height in both phase and amplitude.

DR. MILLMAN. So the 8-hour component is really a part of your model?

DR. ROPER. That is so, although its significance can be gauged from Figure 10, which shows wind spectra determined from the September 1961 data. Since

POWER SPECTRUM, SEPTEMBER 1961
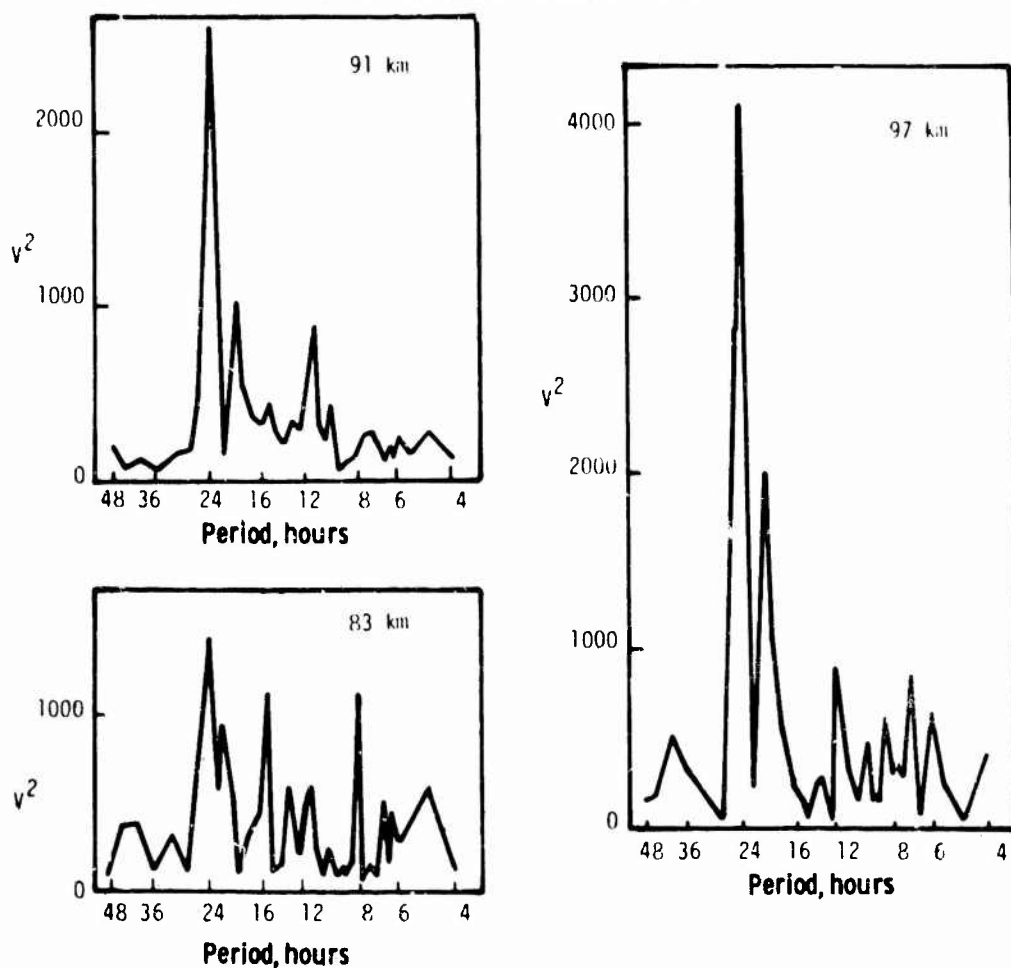
Figure 10. Energy Spectrum, September 1961

V^2 is the sum of the squares of the north-south, east-west, and vertical-wind amplitudes, we call these "wind energy spectra."

As I mentioned yesterday, the model allows for a vertical wind component. In general, we find that the vertical wind has an amplitude similar to that of the errors. It is definitely much smaller than the horizontal wind, as has been appreciated for many years. It appears to have seasonal trends until one puts error bars on the determinations. The vertical winds are not significant.

In each of the Figure 10 plots you can see the amount of "noise" associated with the wind determination. To produce these spectra, we have taken the nine days data, laid it out chronologically, and scanned it to determine amplitudes and phases for periods ranging from 48 hours to 4 hours in increments of .05 cycles per day.

Thus each of these spectra represents 110 scans through the data looking for periodicities between half and six cycles per day. Subsequent to this analysis, we processed each month of the 1961 data over the interval 0.5 to 4 cycles per day, since random scans in the 4 to 6 cycle per day region did not produce any significant peaks.

DR. BARNES. What was the average number of echoes you were receiving per hour during this nine-day period?

DR. ROPER About four per hour. Our new equipment has considerably improved on this rate. In order to improve the statistics, Dr. Elford has treated the whole year's data, and he will present those spectra later.

We are still in the process of interpreting these results. For example, the 16-hour spike at 83 km may be related to the 8-hour component, which also stands out fairly well, although one has to be careful in evaluating the significance of peaks in a spectrum with a fairly low signal-to-noise ratio. At higher altitudes, where we have considerable data, one notices an apparently significant 12-hour component, as well as a slightly more significant 20-hour peak. The 24-hour component stands out well. The 20-hour period is very close to that of an inertial oscillation in the atmosphere at 35 degrees latitude. This was first noticed in our data by Professor Bernhard Haurwitz when Dr. Elford visited him in Boulder early in 1964, although he was not convinced that the significance of the data warranted publication as a detection of an inertial oscillation in the atmosphere at meteor heights.

At 97 km, the noise is again increasing. The anomalous peak at 20 hours is still present, but the 24-hour peak, the most characteristic periodicity of the September spectrum, continues to dominate.

In Figure 11, I have plotted the September 1961 zonal and meridional winds as contours in the height/time plane. As you can see, there is considerable structure allowed within the limitations imposed by our model. The model is designed to cope with a minimum vertical wavelength of 20 km, which is approximately the value for the $S(1, 1)$ diurnal tidal mode which, according to theory, is the shortest wavelength tidal mode. As can be seen from Figure 11 there is considerable structure, particularly in the east-west component around evening twilight. This is interesting in that most rocket vapour trail firings used for determining winds in this region, are fired at twilight. In the data that we at Georgia Tech have from the AFCRL firings there is a bias towards the equinoxial months; and the fact that the vapour trails show considerable structure may be due to their being launched at twilight. There is not, however, any significant difference between evening and morning twilight in the rocket winds.

When using vapour trails, the resolution in terms of determining amplitudes and phases of tidal winds is exceedingly low. One cannot from a single firing
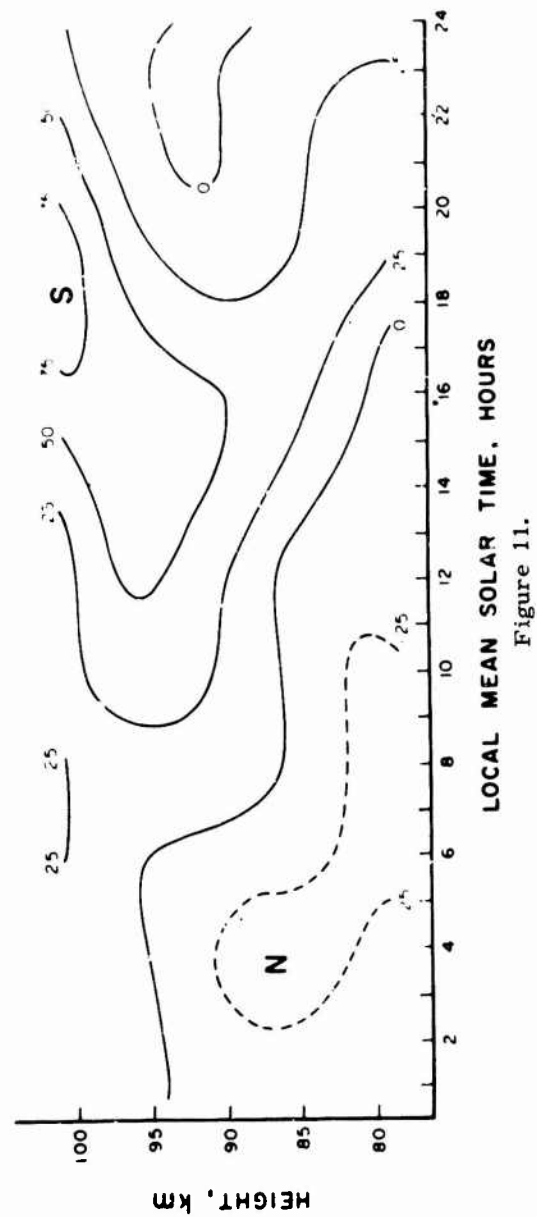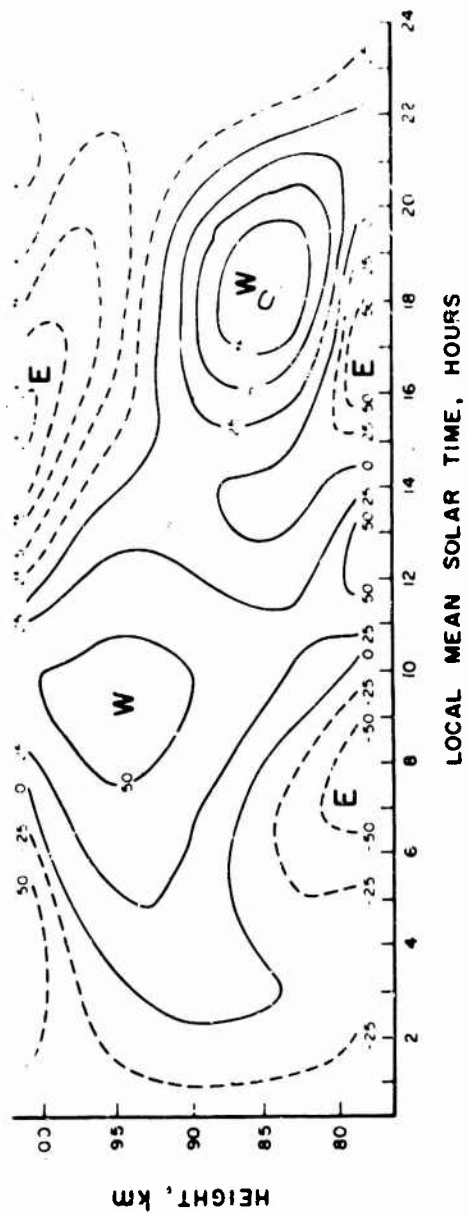
Figure 11.

determine anything about periodic motion with long periodicity. Attempts have been made to extract tidal and gravity wave winds by combining the results of several firings, and these are continuing; but the meteor method stands alone in its ability to resolve prevailing wind, tidal, and turbulent components on a day-by-day basis.

Returning to Figure 11; there is more structure in the east-west than in the north-south component, as is typical of mesospheric and stratospheric winds. Velocities run up to 100 m/sec; this is probably the maximum velocity that one would expect to find in the region. I should point out here, to reduce confusion, that these profiles are constructed from the prevailing, 24 -, 12-, and 8-hour components determined by our analysis.

Well, there is really not much more that I can say at this stage, other than to add that we feel that this use of the analysis, as developed for us by Dr. Gerry Groves of University College, London, has increased the amount of information we can get out of our data. Now that we have access to a large computer, the one hold-up in the system seems to be the film reading. If we had $100,000 for a data acquisition facility, it would be possible to reduce echoes in real time, as Dr. Barnes is already doing, although the matching of the data to the model would still have to be done on a large machine.

If there are any questions, I will do my best to try to answer them.

DR. BARNES. Concerning Figure 10, the energy spectrum, I asked how many meteors you had, and it was four per hour. This was, of course, over the whole height range?

DR. ROPER. Yes.

DR. BARNES. Since you picked three different height intervals, you are going to be running less than one meteor per hour for each spectrum.

DR. ROPER. That would appear to be true, one meteor per hour for nine days at any given height. However, I would like to emphasize one point about this business of matching data against an atmospheric model. Individual echoes are no longer discrete entities in height and time. The line-of-sight drift of an echo measured at 100 km at midnight influences the wind pattern as calculated for 80 km at midday. The model ties all echoes together. We are no longer constrained as we were in the early days of our meteor wind program when we were bound to desk calculators. We are no longer dividing the region into height strata. It is interesting to note, however, that when the Groves analysis was applied to the same data which had been reduced by the old method, it gave the same answers at 80, 90, and 100 km (the midpoints of the three 10 km slabs) within the limits of the errors. The old method proved satisfactory for years, but the Groves analysis gives a more detailed picture.

PROFESSOR PETERSON. You said the diurnal variation in echo rate was two to one?

DR. ROPER. Six to one.

PROFESSOR PETERSON. So the evening twilight period was the place where the model would suffer the most?

DR. ROPER. Yes, particularly at the upper and lower altitudes; however, in the middle height range the rate is still reasonable, and we would consider the 100 m/sec wind velocity to be significant.

DR. MILLMAN. If your analysis can get that out of the low rate, it is to your credit.

DR. ROPER. Not really our credit, I feel, but rather that of Gerry Groves, who developed the analysis for us.

MR. MÜLLER. I have a comment on the speed of reflection point motion; I just happen to have some information here which includes height intervals, and it appears that we have measured an average of about 1.8 km/sec from some two to three thousand echoes a year.

DR. ROPER. As I said, the distribution shown in Figure 2 indicates something like 1 km/sec. It is good to get confirmation from someone else.

PROFESSOR PETERSON. I would like to know for sure what that number means. Many of the echoes don't last a second, and I am wondering is the true value really bigger than this number.

MR. MÜLLER. It is to some extent time-dependent. One does not always get the same value for the gradient if one measures the Doppler change over, say, one second and two seconds on the same echo.

PROFESSOR PETERSON. Do most experimenters measure the change of Doppler beat with time?

MR. MÜLLER. I am speaking of my own results. It is usually a question of, can you observe any changes within half a second. Most will show up within 0.2 of a second.

PROFESSOR PETERSON. But at 100 km altitude many don't last half a second.

MR. MÜLLER. Yes, but at lower heights, many do.

PROFESSOR PETERSON. I don't mean to be critical. I just don't understand what it means.

DR. ELFORD. With the rate of data collection we now have at Adelaide we don't have long enough durations to be able to measure significant changes in the Doppler beat with time.

MR. MYERS. If you can get into the situation where you are only measuring underdense trails and discarding the overdense, long-duration ones, you have simplified your problem somewhat.

PROFESSOR PETERSON. Perhaps for measuring the change of velocity with height.

DR. ROPER. We must not lose sight of the fact that the rate of change of wind velocity with height has to remain the same regardless of what type of echo

is observed, and it is the wind gradient that produces reflection point motion. When we get the Adelaide three-station equipment going again, I am sure that we will measure the same range of shear values, regardless of whether we have long or short duration echoes. This reflection point motion is, as I have said, a problem with long-duration echoes if you wish to know exactly what point on the trail is being observed. If you measure within 0.2 of a second, then your height change is of the order of 100 or 200 meters. This is certainly well within the order of accuracy that we consider adequate in measuring the height for mean wind determinations. If you have to know exactly where you are looking on the trail for a period of time, then you have problems.

DR. SC" .IWORTH. I would like to make one point about the long-duration echoes. It is simply that you are so likely to get an unexpected contribution from another part of the trail that you have no idea whether it is there or not.

MR. MÜLLER. But not within half a second of trail formation.

DR. SOUTHWORTH. The longer you wait, the more you are going to have.

MR. MÜLLER. If you only consider trails which are aspect sensitive on formation, there is always a time-delay before these interferences occur.

DR. SOUTHWORTH. Before you have obvious fading, you have considerably damaged the Doppler data. Therefore, one should work with short trails as much as possible.

DR. MILLMAN. I think it is very important to remember that you have two distinct sets of data. They may conform, but I think in the analysis it is important to make sure that they do. Short trail data will give you measurements made within a small fraction of a second, and all wind velocities will depend upon measurements made in that small fraction of a second. The longer term measurements, which include optical observations of meteor trails and vapour trails released by rockets, can extend over minutes. One must be clear in one's mind that one is dealing with a very different set of data; it is possible that turbulence effects could create differences in the two sets of data. I feel we must bear that in mind when dealing with the wind data.

DR. BARNES. This is very true. Trying to compare some of our decay data with the diffusion coefficients obtained from vapour trails over longer time periods indicates that turbulence is affecting the longer time periods. I didn't realize it was that important with winds determined from long-lasting meteor trails. The point that you have made, that we should look at the short duration trails in order to get the winds, is a very valid one; and the method that Peterson and Nowak have developed at Stanford for obtaining winds close to zero wind speed by putting in this extra 30 or 40 cycles will provide more data.

DR. ELFORD. If you look at the longer duration echoes, you can look at fading; and here you probably get a look at something of the turbulence of the region.

if you are interested in determining the total wind field, including both mean and turbulent motions, rather than looking at only short duration trails, you should look at the relatively long ones also.

DR. BARNES. This goes back to what I said initially, that meteorologists have to get together with the people who are working in this field to determine what they want. If the meteorologists are looking at turbulence, observations should be taken of the longer duration trails, and if they want the mean wind, then observations should be taken of the shorter duration trails.

DR. SOUTHWORTH. I would like to say one thing about long duration trails. I have looked at some overdense trails from large meteors. Unless you have magnificent modeling of the turbulence, you can't say how many centers of reflection you have. Perhaps, over a large number of trails, with a very sophisticated and realistic model, you could get something; but the data is so degraded that I am not going to attempt a statistical analysis.

DR. ROPER. These remarks express my own feelings. As I showed in Figure 3, long duration echoes occasionally have only one or two reflecting centers and the echoes can be profitably analysed. More often than not, one gets the situation I previously labelled "chaotic."

DR. REVAH. Since we are talking about long-duration echoes, I would like to add something to the discussion. We have been observing this type of echo with our forward scattering technique as a means of measuring wind shears at high levels. We have analysed about 37,000 echoes in a year's operation, from which we were able to compute wind shears on 2,000. Although this is less than ten percent, these echoes showed very definite fading, consistent with the existence of two points of reflection on these trails. Multiple reflection is not as rare as one might think. If you have enough data from extended periods of observation, you will get echoes which fade regularly, consistent with reflection from portions of trail on each side of a wind shear. Some of our results from this type of analysis are to be published shortly.

DR. MILLMAN. I would support the view that it is possible to make this type of analysis. We have studied the long-duration echoes obtained at Ottawa using a high-powered, high-resolution radar; and we get two, three, and four reflection points which continue for quite some time, quite consistently. The reflection points are usually separated by the order of a scale height, and this feature of two, three, or four dominant reflecting points in a long duration trail is a quite common feature. We classify them as E-2's, E-3's, and E-4's. We have thousands of records of this type. Unfortunately, analysis of this type has low priority on our data reduction schedule, and we have not gone into the problem in depth, but we do have a lot of data recorded now that gives information on these long-duration type reflections.

MR. MÜLLER. If I may make a point on deriving shears from Doppler records. There is one point that has received very little attention, both here and in the literature, and that is the possibility of detecting plasma resonance in the meteor trail. We realize that we do get resonance in the trail. Theoretically, it will change the phase of the reflected wave by 180°. This produces some effects that might be confusing. Greenhow did some studies many years ago making use of echoes obtained when the wind speed was practically zero, and he did observe phase changes. We should consider this plasma resonance problem. Some people will probably be in more trouble than others, since Kaiser believes the phenomenon is dependent on the peak power of the radar.

DR. SOUTHWORTH. While speaking of resonance, the resonance does change with the ion density and is, of course, different in different parts of the trail. It is highly dependent on the acceleration of the mass in the trail (we have found this to be true of meteors we have observed). The big meteors did not decelerate as they should have in areas of high wind shear, if the data is interpreted literally.

DR. BARNES. Thank you very much, gentlemen. Coffee has been served.

III. Accuracies for the Stanford System

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Stanford University
Stanford, California

When one considers accuracies of measurements in a rather complex system like ours, many influences cannot be determined reliably, among them, interactions between the parts of the system and effects of circuit construction (ground-loops, and so forth). To evade giving a false impression of rigorous results, only first order error effects are being considered here, with the understanding that the measurement accuracies obtained are estimates. Only errors in the new system are discussed. (Note: At the time of the talk, this system had not been used yet to collect data. Operational experience gathered in the meantime has been used to supplement the remarks made at that time.)

Before the measurement accuracies proper are considered, a short description of the station parameters and the expected signals will be given. As mentioned before, the transmitted signal has, for the wind measurements, a peak power of 5 kW with a bandwidth of 7 kHz. In the overhead measurements, half the pulse power, with a spectrum of 200 kHz bandwidth, is used for ranging, and only 2.5 kW peak power is available for the 5-kHz channel. (The amplitude channel was chosen somewhat narrower than the signal bandwidth, to reduce noise.) The spectrum in this narrow band is essentially that of an unmodulated pulse of 0.28 msec duration. If q is the electron line density of the meteor trail, and a normalized line density $Q = \pi r_e q = 0.885 \times 10^{-14}$ electrons per meter is introduced, (where r_e is the classical electron radius) the radar equation yields, for the echo power

as compared with the transmitted power,

$$\frac{P_R}{P_T} = \frac{G_T G_R \lambda^3}{32\pi^4 R^3} Q^2 .$$

Here, G_T is the (power) gain of the transmitter antenna, G_R that of the receiver antenna, R is the range to the trail, and λ the wavelength. It should be noted that $Q = 1$ marks the separation between underdense and overdense trails (which was established around $q = 10^{14}$ electrons/m).

With our parameters for the wind measurements ($G_T = G_R = 36$, $\lambda = 10m$, $R \approx 180$ km), the radar equation leads to

$$\frac{P_R}{P_N} \approx 10^5 Q^2 .$$

Hence, for a trail with line density $q = 10^{13}$ el/m, (i.e., $Q \approx 0.1$) we have an initial signal-to-noise ratio of about 30 dB in the amplitude channel.

In the decay/height measurements, signals are compared from a 2×2 array of dipoles and a single dipole at different heights above ground. Table 1 shows the calculated signal-to-noise ratios of these antennas, as a function of zenith angle. As was pointed out yesterday, our measurement program relates height and echo decay at high elevation angles and uses this "decay height" to obtain the height of winds measured at low elevation angles.

All the signal-to-noise ratios mentioned so far, apply only for the initial echo strength. As the trail expands through diffusion, the amplitude decays -- exponentially in the case of an underdense echo. Doppler and range, however, are measured in the first part of the echo where signal strength is still high.

Two types of errors will be analysed now. One is given through the influence of noise, and in its discussion the signal is treated as continuous. The other kind of error results from quantization as the data are digitized for recording. Since we have 6 bits per character, the pulse amplitude, for example, can only be quantized in 63 steps. As will be shown, it is actually this latter type of error that limits the accuracy of our system.

Let us consider first the error introduced by noise. For the amplitude, we get for the probability density function of the envelope V_t of a sinusoidal signal with amplitude P in narrowband Gaussian noise of power σ_x^2:

$$p(V_t) = \frac{1}{\sigma_x}\left(\frac{V_t}{2\pi P}\right)^{1/2} \exp\left[-\frac{(V_t - P)^2}{2\sigma_x^2}\right],$$

i. e., the distribution is Gaussian. This expression is an approximation for the case of large signal-to-noise ratios and is applicable here.

For the beginning of the echo, with an estimated signal-to-noise ratio of 30 dB, $\frac{P^2}{\sigma_x^2} = 1000$ (i. e. $\frac{P}{\sigma_x} = 32$)' and the probable error (i. e. the standard deviation of the Gaussian process) is $\frac{P}{32}$, corresponding to an error interval of ± 0.25 dB.

The echo amplitude is recorded on a logarithmic scale, so that the expected underdense echo decay yields a linear decay of receiver output voltage.

The above estimate was made for the beginning of the echo, where signal-to-noise ratio is expected to be high. As the signal decays, the influence of noise is going to be greater, but in our determination of decay (for which the amplitude measurement is ultimately needed), this part of the signal is not utilized. We measure decay by finding the maximum amplitude and the point where the signal reaches a certain threshold value (set now at about 15 dB above the noise), and then drawing a straight line through these two points. If the actual signal deviates too much from this line, the echo is not considered in the data reduction. Some accuracy could be gained using a least-mean square fit to the signal, but the expense of this procedure would be substantially higher than that of the present method.

Only underdense trails are wanted for data reduction, and our main criterion for their selection is that the echo decay must be at least half the total duration of the echo. Overdense trails with their nearly constant echo strength followed by a rapid decay are thus eliminated. Trails that lead to signal fading are also excluded from data reduction.

In our case, the point where the signal reaches the threshold can be determined to within plus or minus one pulse, i. e., to within ± 3.3 msec. This error bound was established from a few hundred echoes reduced manually.

In the Doppler channel, we have a sinusoidal signal with amplitude P and phase angle ψ. Gaussian noise of power σ_x^2 is superimposed, and the phase angle of the total signal is ϕ_t. The probability density function of ϕ_t and ψ is

$$p(\phi_t, \psi) = \frac{P\cos(\phi_t - \psi)}{(2\pi)^{3/2}\sigma_x} \exp\left[-\frac{P^2\sin^2(\phi_t - \psi)}{2\sigma_x^2}\right].$$

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Since we are only interested in the distances between zero crossings of the sinus-oidal signal which do not depend on ψ, we can set $\psi = 0$. Then ϕ_t is the phase error of a zero crossing. Since ϕ_t is expected to be small, $\cos (\phi_t) \simeq 1$, and we get a Gaussian distribution for the variable $P \sin \phi_t$.

The Doppler channel has a bandwidth of 80 Hz; hence, noise power in it is 37.5 dB below that in the 6-kHz amplitude channel. Signal power is also dimin-ished, however, since the Doppler frequency is only sampled at 3 msec intervals by the transmitted pulse. The receiver is gated open for a longer time than the length of this pulse, and noise is present for more time than the signal. When the original waveform is recovered by filtering from the samples, the signal power in the recovered waveform is the power of the original signal before sampling (which would have been obtained with cw) multiplied by the ratio of pulse time to the time the noise is present. In our case, this ratio is 0.28 msec/2.2 msec = 0.125; the signal in the Doppler channel is hence about 11 dB below that in the amplitude channel, and a signal-to-noise ratio of 55 dB can be expected for the beginning of the echo. For the first two time-constants of decay, the SNR is better than 40 dB. With this value, $\sin \phi_t$ is smaller than 0.02 with 96 percent probability, i.e., the error in ϕ_t is about 0.6 percent of a half cycle.

Until now, the narrow band portion of our composite signal was investigated. The wideband portion, used for ranging, will be considered next. When the re-ceived signal is phase-demodulated and the running correlation with the transmitted code is performed, the (normalized) correlation function is that of Figure 1 where $T = 0$ denotes the point in time when received and transmitted codes are lined up exactly. (This correlation-function is periodic with the pulse repetition rate, of course, but the range of the meteors is known to within less than this so that no ambiguity exists. Attention will therefore be focused on one transmit pulse alone.)

The correlation function is essentially a triangle of height 1 and base 20 μsec, with some minor peaks, none of which is larger than 1/14. The major peak is the same as that which would be obtained for the autocorrelation function of a single 10-μsec-wide rectangular pulse, and the spectrum of our pulse is hence essentially that of this 10-μsec pulse.

From the radar equation, the signal-to-noise ratio is

$$\frac{S}{N} \propto \frac{P}{kTB},$$

where P is the transmitter power and B, the bandwidth. With a pulse length $t_p \approx \frac{2}{B}$, this becomes

$$\frac{S}{N} \propto \frac{Pt_p}{kT} = \frac{\text{Energy of transmit pulse}}{\text{Noise power per Hertz}} .$$

In our coded pulse, which can be considered as 28 "sub-pulses" of $t_p = 10\,\mu$sec duration, detection is coherent so that signal-amplitude and noise-power in the individual bits are added, and

$$\left(\frac{S}{N}\right)_{28\ \text{pulses}} = 28 \left(\frac{S}{N}\right)_{\text{one pulse}}$$

Thus for the signal-to-noise ratio, the encoded pulse of duration 280 μsec is equivalent to a single pulse of 10 μsec length with 28 times the peak power of the encoded pulse. Conversely, since the broadband signal has roughly 28 times the bandwidth of the narrowband signal, the signal-to-noise ratio in the ranging channel is the same as in the amplitude channel. One effect is neglected here. The arguments given above apply in a strict sense only for a continuous correlation. In our system, the correlation is performed digitally every 10 μsec; in this case at most 3 dB can be lost in the signal-to-noise ratio. This loss is considered small enough to be tolerable. The range is, of course, quantized in 10-μsec steps so that no better resolution than this is possible. If the range value is assigned to the middle of the bit-interval, the resolution of $\pm 5\,\mu$sec corresponds to a quantization error of ± 0.75 km for the range.

DR. BARNES: This goes for an individual pulse?

MR. NOWAK. Yes. This quantization "error" is, in reality, a resolution interval. Errors through noise are generated when some bits of the code are changed, and the correlation does not reach 1 any more. Unless noise is severe, however, the main characteristic of the correlation function, with one major and some minor peaks, is maintained.

In operation, it is not the full correlation that is performed; rather, the number of coincident bits in the two codes is determined. (For the lined-up position of two noisefree codes, the maximum of this number is 28.) With a threshold of 22, above which the number of coincident bits is interpreted as the major peak of the correlation function, a wrong range indication was obtained in practice in 0.4 percent of all cases.

Quantization errors introduced by the conversion of the analog values into 6-bit digital characters will now be considered. In the ranging, the quantization conforms with the pulse coding itself, and no additional error is introduced. In general (with the exception of the few cases where the code is influenced by noise so that it appears shifted), noise in the ranging waveform will lead to a loss of range indication rather than a wrong value. Hence, range is measured to within

0.75 km. Since, in our measurement programs, we measure range only at fairly high elevation angles, the error is translated into an almost equal height error. At 25 degrees zenith angle, the height error introduced by range inaccuracy would be maximally ± 0.68 km; at 40 degrees zenith angle, ± 0.575 km. The mean square distortion of the range values, considering the quantization error alone, would be $D^2 = 0.0833 \, E_q^2$ (where E_q^2 is the quantization step, in our case 1.5 km), i.e., the standard deviation of the range measurement is $\sqrt{0.0333} \times 1.5$ or ± 0.43 km.

For the measurements of the echo amplitude, we are operating with a dynamic range of 40 dB, for which the 63 steps of the 6-bit digital character are available; a value of 0.8 dB per quantization level was chosen. Above, an error of ± 0.25 dB was established for the amplitude error through noise, at the beginning of the echo. The standard deviation of the signal with errors from noise and quantization is going to be ± 0.3 dB.

In the angle measurement, the receiver is switched alternately between the two antennas, and a pulse amplitude from one antenna is compared with the arithmetic mean of the adjacent pulses from the other antenna (since, from one sample to the next, the receiver output voltage decays linearly). Neglecting the noise in the pulses, which is much smaller than the quantization error, we know the voltages from each antenna to within ± 0.5 counts; i.e., the difference can be wrong by ± 1 count, corresponding to ± 0.8 dB.

For zenith angles smaller than about 40 degrees, this error in the difference of receiver voltages (i.e., with the logarithmic gain, in the ratio of the echo amplitudes from the two antennas) leads to an angle error of about ± 0.8°. At 25° zenith angle, this angle error causes a height error of about 0.65 km; at 40° zenith angle, about 1.2 km. Combined with the range error, these values should lead to a total height error of about ± 1.5 km, in the decay-height measurements.

The Doppler measurement also has a quantization error associated with it. Sixty-three counts are available to measure the times between zero-crossings of the Doppler signal. With an offset of 40 Hz, we expect Doppler frequencies between 20 and 60 Hz. The counting frequency was chosen as 2.5 kHz, so that 62.5 counts are available for the half period of a 20-Hz signal; with a quantization error of ±1 count, an error of ± 0.3 Hz is made here when one half period is measured. At 60 Hz, there are 21 counts per half-period, and the measurement error is about ± 2.5 Hz. These values are seen to be much larger than the errors introduced by noise, and the latter can be neglected. Since 5 consecutive half-cycles are measured, the mean value of the half periods has an error $\sqrt{5}$ times smaller than these, i.e., ± 0.13 Hz at the lower frequency limit and ± 1.1 Hz at the upper one.

It was mentioned several times that the observed Doppler signals have different half cycles. I observed the same effect in my records, but the averaging over several half periods should overcome this problem.

MR. MYERS. The error is about 5 m/sec, then?

MR. NOWAK. Yes. At 30 MHz, the Doppler shift is given in Hz by approximately v/5, where v is the observed wind speed in m/sec. Sixty Hz in our offset signal corresponds to a Doppler shift of 20 Hz, which in turn would indicate a wind speed of 100 m/sec. With an error of 1 Hz in the Doppler frequency, the error in wind speed is 5 m/sec.

The system was actually built around the digital recording device and has its limitations. As was shown above, the quantization error is the largest error contributed to the measurements. An improvement is possible by recording each measured value not in one, but in two characters. To do this without excessively large memory circuits, the digital recorder would have to step at twice the pulse rate of the radar. Since the stepping rate of the recorder is limited to 500 Hz, our pulse repetition rate would have to be lowered to 250 Hz, with some loss in signal resolution.

THE FLOOR. Are your figures on signal-to-noise ratio based just on the amplifier noise, or did you include more?

MR. NOWAK. The main contribution to the noise used in the calculation was cosmic noise; 1 assumed a sky temperature of 30,000 degrees.

THE FLOOR. You have discussed errors in ranging and elevation. When you measure elevation, do you not also measure range at the same time?

MR. NOWAK. Yes. As l said, we have an error in range of ± 0.75 km and an error from the angle measurement that translates into about ± 1 km height error. The composite error in the height measurement due to the inaccuracies in range and elevation angle, is then about ± 1.5 km.

THE FLOOR. Why are you measuring diffusion coefficient to measure height?

MR. NOWAK. We are measuring density* (i.e., decay of the meteor echo) at fairly high elevation angles, where our range and angle values give a fairly accurate height determination. Thus, we obtain a diffusion coefficient as a function of height. Since at these high elevation angles, the angle between radar ray and wind is fairly big, the Doppler measurement is quite inadequate. Furthermore, at the moment we do not have any azimuth information in the "overhead" measurements. Hence, winds are measured at low elevation angles with known azimuth, but neither range nor elevation angle is determined. Rather, the height is found from the decay, using the decay/height relationship established in the "overhead" measurements.

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*For a complete description of the Stanford Meteor Radar, see Final Report on Air Force Contract AF 19 (628)-3996, (AFCRL-67-0347), No. SEL-67-046, May 1967, "An Integrated Meteor Radar System for Wind and Density Measurements in the Upper Atmosphere" by R. Nowak.

Table 1. Power Gain of a 2 X 2 Array and a Dipole,
and Signal-to-Noise Ratios of the Two Antennas as
a Function of Zenith Angle θ in the Direction of
Gain Maximum

θ	Power Gain		Signal-to-Noise Ratio (dB)		Signal Ratio (dB)
	$G_{2 X 2}$	G_{dip}	$SNR_{2 X 2}$	SNR_{dip}	$S_{2 X 2}/S_{dip}$
10	16.2	0.02	40	10.9	29.1
20	12	0.25	36.8	20	16.8
30	7.4	1.12	31.5	23.3	8.2
40	3.6	3.06	23.5	23.0	0.7
50	1.27	5.5	12.5	18.9	-6.4
60	0.27	6.8	-4.4	9.6	-14.0

Note: A comparison of signals from the two antennas (given in the last column)
shows strong dependence of the signal ratio on he elevation angle.
Signal-to-noise ratios are for Q = 0.3.
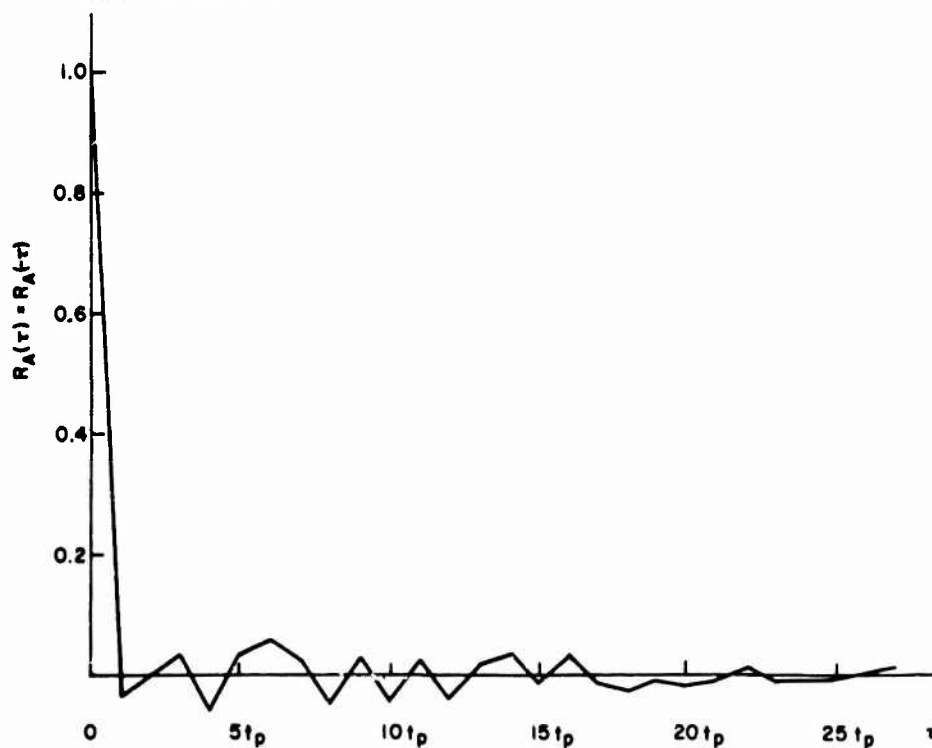
Figure 1.

IV. The AFCRL System, I

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1. INTRODUCTION

It is a pleasure to be here before this distinguished group. I just want to mention that I am glad the weather has changed; otherwise you wouldn't believe what I did over the weekend. Two mornings in a row I was out there watching the Perseid shower. In one-half hour I saw 20 meteors on the 13th and 11 on the 14th.

I want to thank Bill Ramsey, and I don't see him here, for the help I got on this paper. You would be surprised at the similarity between my paper and his notes.

We will take this in order. In order to compute the winds, we know we must have the elevation angle. To get the height of the wind, we need the elevation angle and the range. So we shall take the accuracy of these quantities in order.

2. RANGE

We have data that comes in two different forms, photographic and output from the computer, and each of these is subject to its own type of error. We will consider first the range. In this, there is an error due to noise. This is an error introduced by the fact that we have a finite rise-time.

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Figure 1 shows the leading edge of one pulse. The amplitude is A, n(t) the noise level, T_R is the rise-time, and ΔT_R the timing error due to noise. Now, from similar triangles we can compute the uncertainty in time, ΔT_R, as a function of the signal-to-noise ratio and the rise-time. The equation is repeated in Figure 2, where the signal-to-noise ratio is given by S/N.

Now, the rise time, as you know, depends on the bandwidth. In our case, this is equal to 0.3 MHz, giving a rise time of 10 μsec. Substituting in the equation for ΔT_R and using the fact that our signal-to-noise ratio varies from 15 dB to 40 dB (power ratios of 30 and 10^4 respectively), we get the two results shown in Figure 2. In the case of 40 dB, we have an error of 0.15 m, and in the case of 15 dB, the error is 50m.

Now, this would be a very good place to end the discussion of errors in range. However, we know that there are other errors. One of these is due to timing. If we look at Figure 3, we see that the signal is sent out from the transmitter and the timing is obtained by a 124-kHz clock, with an interpulse period of 8.06 μsec. We time the return of the pulses, noting that the return can come in any time during that interval of 8.06 μsec. This is equivalent to 1.2 km, which is the largest error for a single pulse. We take a large number N of pulses, and divide the error in a single pulse by $\sqrt{N}$. In our case, the usual number of pulses is about 50, so that our error is about 0.2 km. We consider only those range counts, or timing pulses, which fall in the interval $\pm$ 2 counts of the initial count, and continue taking range counts until four are obtained which fall outside that interval. Thereupon the range is computed by substituting the mean count, K, in the equation

R(km) = 1.201K - 12.2.

3. ELEVATION ANGLE

Going to the elevation angle, one of the errors is due to quantization. In our case, the signal levels are converted to seven-bit words, which have quantization increments of 80 mV.

Figure 4 shows the pertinent quantities. In our case, the quantization increment is approximately 80 mV, and the angular sensitivity is 3.5°/dB. Now, the output-to-input sensitivity is 0.1 V/dB in the log mode, and 0.125 V/dB in the linear. Therefore, the errors, $\Delta\theta$, in elevation angle are 2.8° and 2.2°. Again, if we take N pulses, these errors are reduced by the factor $1/\sqrt{N}$.

There is another source of error due to the antenna patterns, the lower of which is more sensitive to man-made noise. This becomes more serious as the signal becomes weaker, so the elevation angles read too low. Now, this could be

$$\frac{n(t)}{\Delta T_R} = \frac{A}{T_R}$$

$$\Delta T_R = \frac{T_R}{A/n(t)}$$
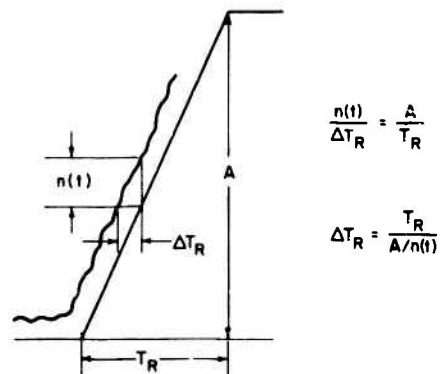
Figure 1. Error (Range) Due to Noise

$$\Delta T_R = \frac{T_R}{S/N} \qquad T_R = 10\,\mu sec$$

S/N (dB)	ΔT_R (μsec)	ERROR (m)
40	10^{-3}	0.15
15	0.?	50

Figure 2. Error (Range) Due to Noise (Contd)

8.06 μsec OR   1.2 km
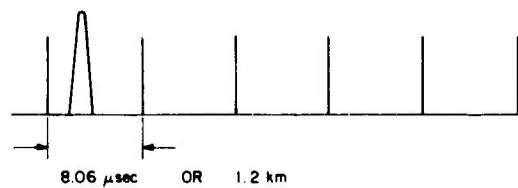
$$PROB \ E = \frac{1.2}{\sqrt{N}} = \frac{1.2}{\sqrt{50}} = 0.2 \text{ km}$$

Figure 3. Error Due to Time-Resolution

$\Delta E = 80\,mV$

ANGULAR SENSITIVITY = 3.5°/dB

	LOG MODE	LINEAR MODE
$\dfrac{\Delta(O)}{\Delta(I)}$	0.1 $\dfrac{VOLT}{dB}$	0.125 $\dfrac{VOLT}{dB}$
$\Delta \theta$	2.8'	2.2°

Figure 4. Quantization Errors

reduced by setting a minimum level below which we would not consider the return. This is not desirable when you consider the large gaps that we have in the data.

Another assumption made is that the ambient ionospheric electron density is not great enough to give a refraction of the beam at our operating frequency (36.8 MHz). This seems to be a reasonable assumption, considering the electron concentration below our maximum height.

A few words on the calibration of our system. Dr. Barnes might care to develop it further. We calibrated the elevation angles by taking a large number of readings. Then, assuming the mean height of meteor trails to be 93 km, we selected the best fit for the elevation angle against the data from the antenna. We modified this, so that we have the elevation angle within three degrees. We're still working on this, and hope to refine it further to reduce the error to less than one degree.

4. HEIGHT

Next we go on to the errors in height. Figure 5 shows the equation used in computing the height. Here, a is the radius of the earth, r is the range, and θ is the elevation angle. Throughout, we use the elevation angle rather than the zenith angle. The error is found by taking the derivative of h. The final form for dh is obtained by using the fact that h is very small compared to a. The substitution in this equation is made in Figure 6, where I have tabulated the quantities to show the relative contributions of the various terms. That value of 1.2 km we have for dr is the error we found in the discussion of the range, and when I say error, I use that interchangeably with uncertainty. We consider the uncertainty in elevation angle to be 2.5° at 20 degrees, and 5° at 70 degrees. We see that the error in height due to an error in the range is less than the error due to the elevation angle. At 20 degrees it is 0.5 against 11.8, giving a total of 12.3 km. At 70 degrees it is 1.1 compared to 2.6 for a total of 3.7 km. If we assume that the uncertainty decreases by a factor of $1/\sqrt{N}$ for N pulses, then we see that to measure h to within 1 km, we must have 152 pulses at 20° and 14 pulses at 70°.

The equation for the height is based on the assumption of a spherical earth. Ignoring the curvature of the earth is not too serious at high elevation angles, but once we get down to 20 degrees we find that at a range of 300 km the error in height is 6 km, which is not negligible.

$$h = \sqrt{a^2 + r^2 + 2ar \sin \theta} - a$$

$$dh = \frac{\partial h}{\partial r} dr + \frac{\partial h}{\partial \theta} d\theta$$

$$= \left(\frac{r}{a} + \sin \theta\right) dr + r \cos \theta \; d\theta$$

θ	r(km)	$d\theta$	dr(km)	$\frac{\partial h}{\partial r} dr$	$\frac{\partial h}{\partial \theta} d\theta$	dh(km)
20°	287	2.5°	1.2	0.5	11.8	12.3
70°	85	5.0°	1.2	1.1	2.6	3.7

Figure 5. Error in
Heights

Figure 6. Error in Heights (Cont'd)

5. VELOCITY OF THE WIND

In computing the wind, we use the body Doppler. Figure 7 shows the Doppler equations of physics. In the first equation, we assume that the transmitter is a stationary source, and the meteor trail is a moving observer, so that a positive sign means the meteor trail is approaching the source. In the second equation, we consider the reflecting meteor trail as the source, and the transmitter is the stationary observer. Here, a plus sign means motion away from the transmitter. By eliminating f_2, the frequency at the trail, we get the third equation. Here f_1 is the transmitted frequency, and f_3 is the frequency of the return signal at the receiver. If we beat these two frequencies together, we get f_d, the Doppler frequency used in computing the wind. This is given by the first equation in Figure 8. In this equation, we substitute the expression for f_3 from Figure 7. By making use of the fact that the drift of the meteor trail is much less than the speed of light, we get the second equation of Figure 8. On making the indicated substitution for c the velocity of light, and solving for the line-of-sight component S_R of the wind we get the third equation.

In practice, f_d can be measured either by counting the number of pulses in one Doppler cycle, or by measuring one Doppler wavelength in arbitrary units. Figure 9 shows the relation between the two methods. Our pulse recurrence frequency (PRF) is 500; N is the number of pulses in one Doppler wavelength; x is the length of one Doppler cycle in arbitrary units; y is the length of the oscilloscope sweep in the same units; and the time of the sweep is 1/2 second.

Because the equations do not tell us whether the radial wind is toward or away from the station, some method must be used to resolve this ambiguity. The method used is the one already mentioned by previous speakers, namely, to beat the incoming signal against the reference signal in two detectors, one of which has a phase retardation of 90°. Then each detector will have its Doppler beat, and one or the other will lead, depending on whether the wind is blowing toward or away from the station.

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$$f_2 = f_1 \left(1 \pm \frac{V}{C}\right) \quad \leftarrow \text{TOWARD SOURCE}$$

$$f_3 = \frac{f_2}{1 \pm \frac{V}{C}} \quad \leftarrow \text{AWAY FROM OBSERVER}$$

$$\therefore f_3 = \frac{f_1 \left(1 + \frac{S_R}{C}\right)}{1 - \frac{S_R}{C}} \quad S_R \text{ TOWARD STATION}$$

Figure 7. Doppler Equations

$$f_d = f_3 - f_1$$

$$= \frac{2 f_1 S_R}{c} \qquad \left(\frac{S_R}{c} << 1\right)$$

$$S_R = \frac{f_d \lambda}{2} \qquad (c = f \lambda)$$

Figure 8. Doppler Equations (Cont'd)
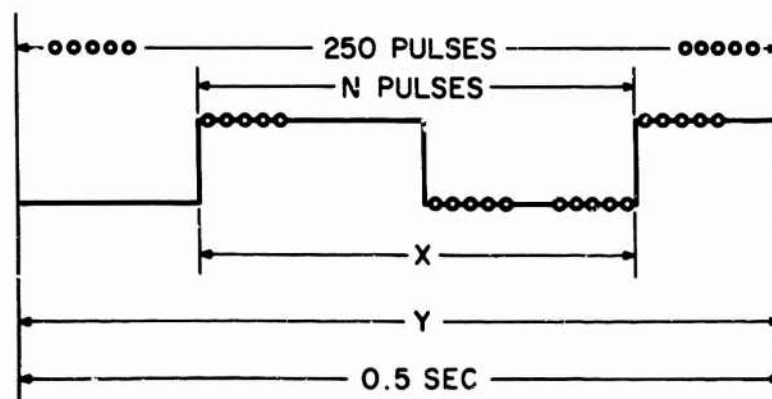
$$S_R = \frac{f d \lambda}{2} = \frac{PRF}{N} \cdot \frac{\lambda}{2} = \frac{Y \lambda}{X}$$

Figure 9. Doppler Wind Equations

Incidentally, when Dr. Grossi yesterday mentioned interchanging wires and getting the wrong results, he struck a rather nostalgic note. We had similar trouble, and had some very confusing moments because of this very error.

Now, referring again to Figure 9, we can see what some of the sources of error are. For example, a variation in the frequency will give an error by giving us the wrong value of λ. In our case, the variation was about 3 kHz in 36.3 MHz, or less than 0.01%.

There is another source of error in taking the Doppler off the photograph, because of the finite size of the beam spot. Its position can be estimated to about 0.05 cm. For a base line of 10 cm, this gives an error of about 0.5%. In addition, there is an error of 2% to 5% in the time base.

Next consider N, the number of pulses in one Doppler cycle. We find that the error is 1 or 2 pulses at the most. Figure 10 shows the percentage of error in S_R, given an error of ± 1 and ± 2 in N. The dashed lines are negative errors.

In computing the wind velocity, it is assumed that all the trails we look at are to the north or west of the station. Because of the wide antenna pattern in the horizontal plane, the error in an individual trail can be very large, hence no computation is made of the percent error. However, for a large number of trails, the mean azimuth angle will be that of the antenna, especially since that is the direction of the strongest emitted power, and therefore, of the greatest probability of signal return.

Another assumption we made is that the winds are horizontal. This has been shown to be true to within a few degrees. The assumption is certainly good at low elevation angles. Figure 11 shows how the error increases with increasing elevation angle. Here, α is the angle the true wind, V, makes with the horizontal; and V_H is the assumed horizontal wind. It can be seen, that even for winds inclined 10 degrees to the horizontal, the error is less than 35% for elevation angles under 60°, but above 60°, the error increases rapidly with higher elevations.

Besides actual errors in measuring the wind, there is a bias against light winds. The theoretical lower limit of S_R is given by one complete Doppler cycle on the oscilloscope. Referring to Figure 9, we see that, since the time base is 0.5 sec, and the PRF is about 500, N = 250, so that S_R = 8 m/sec. In practice, the bias is more serious, because the typical trail is only about 0.2 sec in duration. For this, N = 100, and S_R = 20 m/sec for the lower limit.

There is no such difficulty at higher values of S_R, since the upper limit is given by 4 pulses in one complete cycle. Then, for the highest Doppler velocity we can measure, we have 509 m/sec, which is well above the 200 m/sec that we set for our top value of wind speed.
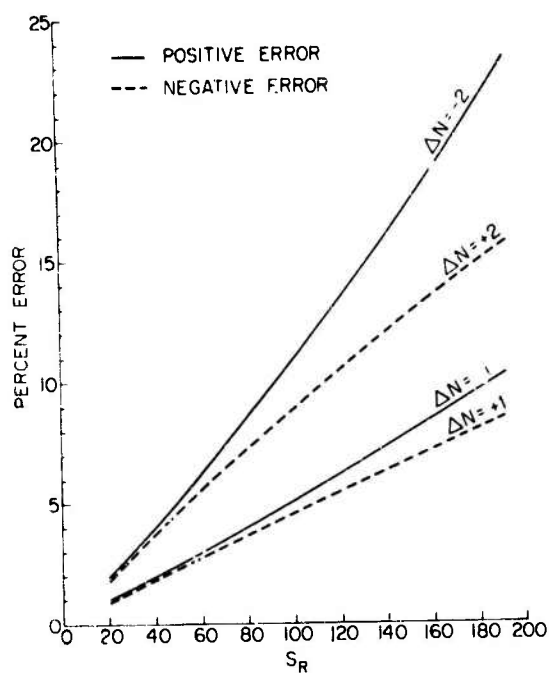
Figure 10. Error Due to ΔN
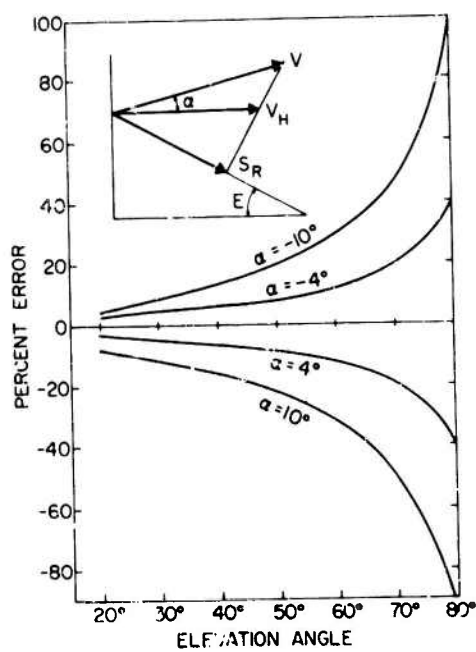
Figure 11. Error in Nonhorizontal Winds

V. The AFCRL System, II

Dr. Arnold A. Barnes, Jr.
Air Force Cambridge Research Laboratories
Bedford, Massachusetts

DR. BARNES. Thank you, Joe. I think we will hold off on the question period until I have finished my part of the presentation. I am going to discuss, primarily, some of the problems that we have had and also data reduction methods. It was with the data reduction that we felt we should concentrate our effort because there are other people more qualified to make the radar set.

Figure 1 shows the AFCRL transportable system. The transmitter, the receiving equipment, and the computer are housed inside the trailer. The ground screen, above the trailer, is 50 feet by 100 feet. This screen is composed of copper wire with a steel core, and the wires are located approximately a foot apart in each direction, so we have a nice square grid. We have two antennas placed at a half wave length and a quarter wave length above the ground plane. These allow us to measure the elevation angle. The antennas are rotatable. We rotate them so that we can look north for a half hour, and then we rotate them so we look west for a half hour.

Behind the trailer there are two towers for the transmitter. We had so much trouble with the rotator that we put in a second tower. One now points north and the other one west. The antennas are switched or rotated in unison by the antenna programmer.

Figure 2 clearly shows the noise problems that we have been having. To the right is the AFCRL parking lot, and at eight o'clock in the morning you can't get

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anything because of the ignition noise. Behind the trailer, we have Lincoln Laboratory which produces a good bit of noise. Route 128 is in the background. I don't really know if I am receiving any ignition noise from 128, but I tend to blame at least some of the troubles on the noise from it.

Figure 3 shows the inside of the trailer. From left to right are: the air-conditioning equipment, the power supply, transmitter racks, receiving racks, data-processor, the film recorder, and the rack holding the computer, the digital converter, and other equipment necessary to run the computer system. The two Flexowriters are used to control the computer.

The information fed into the computer through the 44-bit buffer is given in Table 1.

Table 1. Input Information

Quantity	Buffer Bit Positions
Valid Pulse	0
Direction North	1
Direction West	2
Time	3-10
Doppler A	11
Doppler B	12
Normal Video	15-21
Range	22-29
Elevation A	30-36
Elevation B	37-43

The PB-250 is a 22-bit word computer, so the 44 bits are handled as a double precision word and the above information appears as the following two consecutive words stored in the PB-250 memory (Figure 4).

The valid pulse is actuated in the following manner. A return above the threshold value is obtained in the range 60 to 300 km. A range bin, approximately 4 km wide, centered on the 1st pulse is then tested on the succeeding M pulses. If N of the M returns have signals in the bin exceeding the threshold value, the valid pulse is changed from 0 to 1. (We use N=9, M=12.) The valid pulse actuates the scopes and is used by the computer as a signal to store the data from the next 246 pulses in the computer memory.

If the antenna programmer is operating and the antennas are pointing north the 1-bit is a one and the 2-bit is a zero. If the antennas are west and the programmer

Figure 1. Aerial View of the AFCRL Radar Meteor Trail Set in Lexington, Mass.
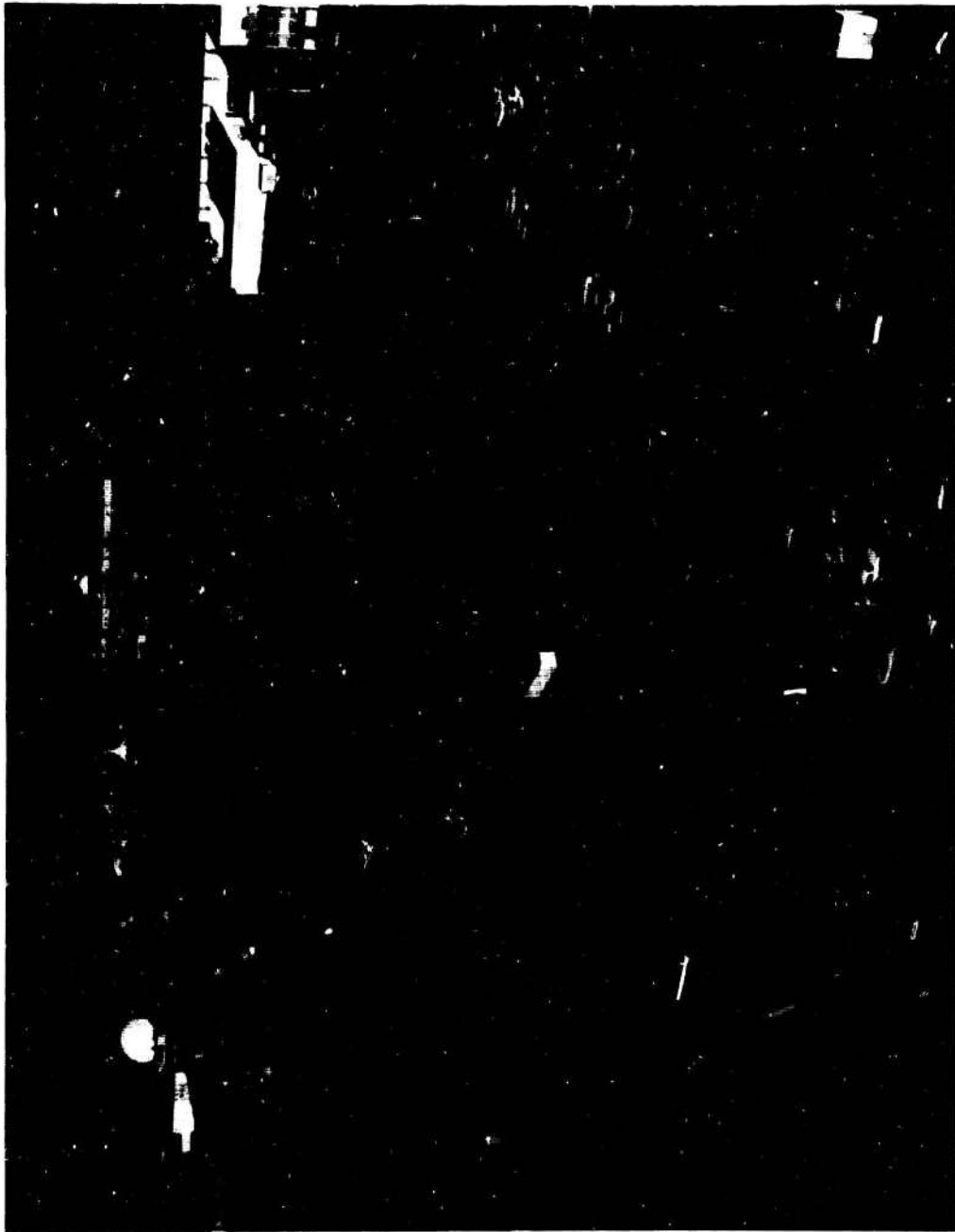
Figure 2. Aerial View of the AFCRL Radar Meteor Trail Set in Lexington, Mass.
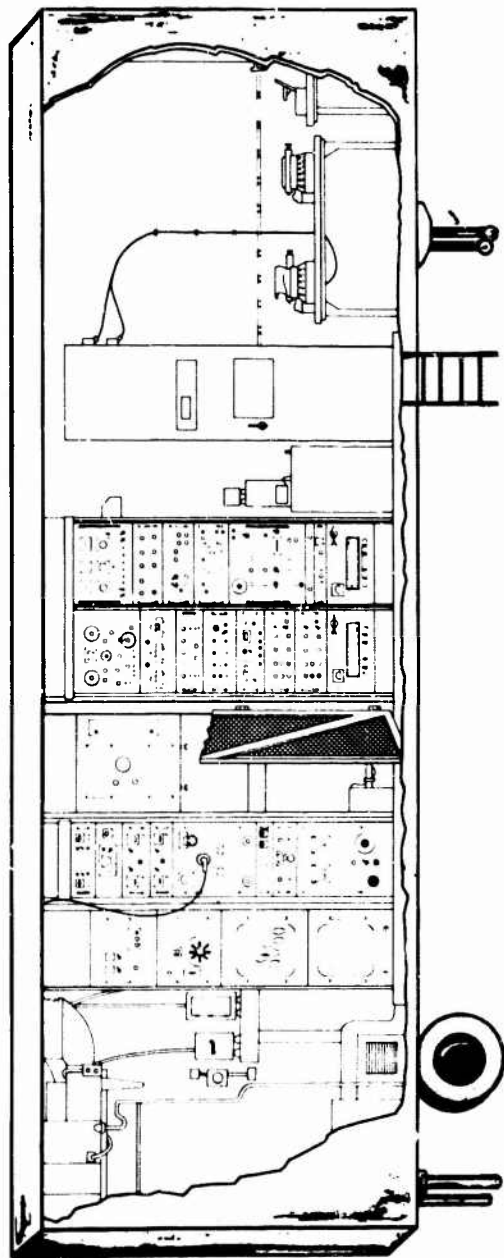
Figure 3. Interior of the AFCRL Radar Meteor Trail Van

1ˢᵗ WORD

RANGE	ELEVATION A	ELEVATION B

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21

2ⁿᵈ WORD

V ALT D	B I T	DIREC-TION	TIME	DOP A	DOP B	NOT USED	NORMAL VIDEO

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
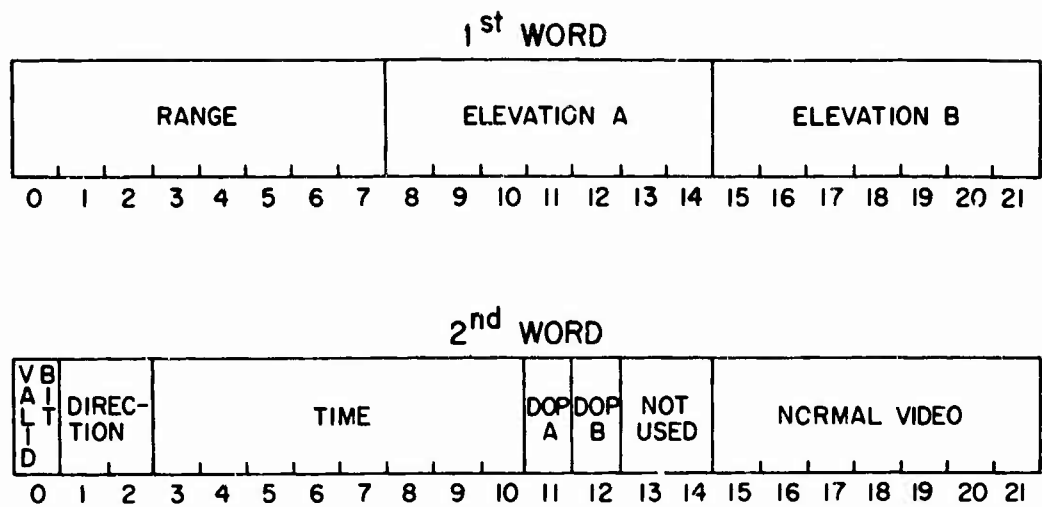
Figure 4. Data Return Format

is on, the 1-bit is a zero and the 2-bit is a one. Zero-zero and one-one are used for other directions.

Time is given to the nearest tenth of an hour by a special digital clock which resets to zero at 2400 hours.

Doppler A and Doppler B have only two values each as explained by Mr. Ramsey yesterday.

The normal video, elevation A, and elevation B are outputs from 3 receivers. They are multiplexed through an A to D converter and stored in the buffer. The trigger from the computer resets the range counter which actually is counting the beats of a 127 kHz oscillator. The counter is halted when a signal from the 60 to 300 km ranges exceeds the threshold level. Each count is equivalent to 1.2 km as Joe explained.

David J. Galas, now of the University of California, developed the formula we use for converting from the clock counts to range and also corrected the discrepancies in the film-derived range, while he was on special assignment to AFCRL from the Air Force Academy during the summer of 1965.

I wrote a program which controls the radar set and stores the information in the computer. At the beginning of the cycle, the computer sends a pulse out which is used to trigger the transmitter. The computer is controlling the radar set so time is controlled inside the computer. The trigger also resets the range counter and all the other components that must be started.

At the end of approximately 2 msec the information is available at the buffer register. We then give a command to read this into the buffer register and then another command to read the information from the buffer register into the digital computer. So, at the end of each pulse, the return data shown in Figure 4 is read into the computer before the start of the next pulse.

If the valid pulse has changed to the one level, then the information is stored, and the computer changes the program slightly so that the information received from the next 245 pulses is also stored in the computer. Otherwise, it continues right through the loop waiting until it receives a valid pulse. Once a valid pulse is detected, it will store the information for the next half a second (some 246 pulses) in the computer.

We have a program which will print the stored information on the Flexowriter, so that we can check out the whole system right then and there. Figure 5 is an actual printout taken in order to check the system on a real return. The range is given in terms of the range count, not in kilometers. We calibrate at the beginning of each run. For August, we calibrated about the sixth of August; and I will proba-bly calibrate after this meeting is over, so that we will know how to convert from the numbers in the computer to dB for normal video, elevation A and elevation B.

METEOR TRAIL OBSERVATIONS
DIRECTION N   TIME 207

RANGE	EL.A	EL.B	DA	DB	VIDEO
123	0	6	1	1	48
123	86	78	1	1	49
123	0	6	1	1	50
123	85	80	1	1	51
123	0	6	1	1	50
123	80	74	1	1	50
117	0	6	1	1	11
123	91	89	1	1	50
123	0	6	1	1	50
123	91	91	1	1	50
124	0	6	0	1	48
123	84	86	0	1	47
123	0	6	0	1	48
123	96	89	0	1	49
123	0	6	0	1	49
123	74	73	0	1	48
123	0	6	0	1	51
123	88	86	0	1	51
124	0	6	0	1	52
123	83	80	0	1	49
123	0	6	0	1	50
123	84	77	0	1	50
123	0	6	0	1	50
124	84	82	0	1	47
123	0	6	0	1	48
123	89	83	0	1	50
123	0	6	0	1	50
123	85	85	0	1	49

Figure 5. Flexowriter Printout of Real Trail, July 1966 (A.A. Barnes)

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Dopplers A and B are in zero or one state for each pulse. We get the ampli-
tude of the signal from the normal video for computing density. There is redun-
dancy here. We could take the amplitude from elevation A or B if we desired,
but because we went on the film first and it was needed on film, it is available.
Notice that from elevation A and B, they are taken only every other pulse. This
is the result of our method of placing the information on the scope for filming.

Figure 6 is a trail that was taken back in 1964 on film. I would like to relate
what is on film to what is available in the computer. The time, 1356, is at the
top. The elevation A and elevation B are plotted against each other as the x and y
of the top half of the display. The origin is in the right center and the slope of
the line through the dots, from the origin towards the upper left, is used to obtain
the elevation angle. The dark line across the center is composed of range markers
which are 4 km apart going from approximately 60 km on the right to 300 km on the
left. Range to the meteor is indicated by a larger dot about a third over from the
right. Doppler is recorded as the horizontal, broken lines below the range mark-
ers. Normal video amplitude, below the Doppler, decreases in amplitude from
right to left until it is lost below the threshold level.

All of this information is also stored in the computer. Notice the noise on the
film after we lost the trail. All of this noise is stored in the computer but is dis-
carded in the computations.

Figure 7 is an overdense trail observed at 1102. Note that the signal level re-
mained above the threshold value for the full half second and Doppler information
also was obtained for the full half second.

As I mentioned before, the radar data can be handled by the on-line computer
to provide real time output, or the data can be edited and recorded on magnetic
tape for further processing on another computer. At the present, I am the only
PB-250 programmer available. Because I don't want to do all the programming
myself, and since I can get programming help from the AFCRL computation center
if I use their IBM 7044, I have written a PB-250 program which edits the radar
data and puts it on magnetic tape which can then be run on the IBM 7044.

To date, two programs have been written by the computation center. The first
provides the information on a pulse-by-pulse basis similar to the Flexowriter
printout shown in Figure 5. This program provides a check on the complete sys-
tem right up through the IBM 7044. It has also provided us with information on
the pulse-to-pulse variability of the recorded quantities, which is needed to develop
the data reduction programs. The second program selects usable trails, reduces
the data, and provides wind, height, and other data on punch cards. Each line
(card) in Table 2 lists data from one meteor. This program is an operational pro-
gram and it was used this morning to obtain the wind data from meteors which were
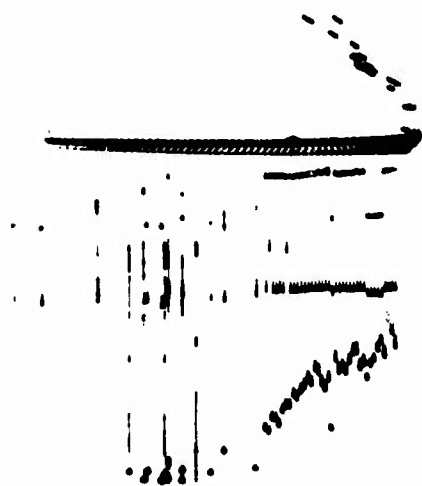
Figure 6. DST, 9 August 1964,
Lexington, Mass., 36.8 MHz Log
Elevation Angle, Time: 0.5 sec,
200 pps, 100 μsec
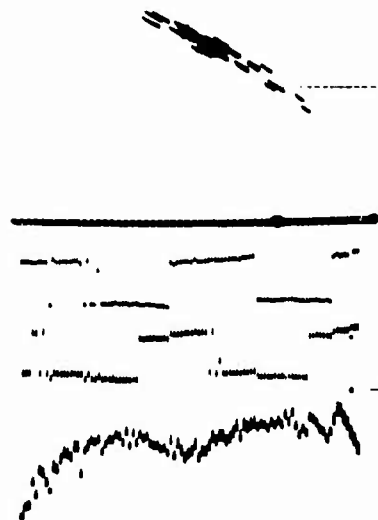
Figure 7. DST, 9 August 1964,
Lexington, Mass., 36.8 MHz
Log Elevation Angle, Time: 0.5
sec, 200 pps, 100 μsec

recorded on magnetic tape over the weekend. As I mentioned before lunch, the
tape was taken off the radar set this morning, submitted to the AFCRL computation
center, and provided me with wind data before noon.

Tape number 10 output, shown in Table 2, contained 139 records. Each record
contained one-half second of radar data which had passed certain screening per-
formed in the PB-250 to separate the meteor trails from "noise trails." Further
screening performed in the 7044 rejected the first six records. Record 7 contained
usable wind information which was punched on the first card. From left to right we
have the date, 16 December 1965; the time, 9.9 hours (time is measured to the
tenth of an hour); tape identification, TAP 10; range, 127 km; azimuth angle, 330°;
elevation angle, 34.8°; equipment identification, TS; radial direction and speed,
- (blank), horizontal wind component, V+ (blank); height of observation, 73 km;
and the record number, 7. The second number after the decimal point in time is

used to identify more than one usable wind obtained in a six-minute period. The
TS in the identification columns is used to indicate equipment or other changes.
A minus radial direction means that the trail is drifting away from the radar set.
The speed is in meteors per second. When the speed is low and the trail lasts for
only a few tenths of a second, we often obtain the direction but not the speed. The
radial speed is changed to a horizontal wind component on the assumption that the
vertical motions are zero. We have taken some liberties by taking the 330° azi-
muth as north and hence calling these winds V winds. The height, 73 km, is cal-
culated for the round earth using the range, 127 kilometers, and the radar deter-
mined elevation angle, 34.8°. We are presently working on a third program to
obtain decay rates and derived density information which will go into some of the
blank columns seen in Table 2.

MR. NOWAK. I have a question concerning the problem of the reduction of
quantization error by observing several values. Now, if you don't have any jitter
in your return signal, if your return signal is, let us say, an ideal signal, and you
take the quantization error, you get the same quantization error at the same out-
put for each pulse, and no matter how many pulses you take, you always get the
same error. I am not quite sure that I understood the point that was brought up
here that the error in the quantization error could be reduced by looking at several
returns. Maybe I misinterpreted that.

DR. BARNES. This was something that Joe brought up?

MR. NOWAK. Yes.

DR. BARNES. Let's take the range. With the range, the 127 kHz clock is
not synchronized with the computer. Therefore, you have a random start, and
you always have a random end. Because you have a random start and random end,
it turns out that this actually helps in this case. So that you do obtain better range
information. This was one of the mistakes that we made that turned out to be in
the right direction. We didn't tie the ranger counter into the computer.

MR. MÜLLER. What is your spread in azimuth angle on one elevation angle
aerial?

DR. BARNES. The 3-dB widths for the free space patterns as provided by
the vendor are 100° along the horizontal and 60° in the vertical. According to
theory, if the antenna is placed above an infinite ground plane, the beam width
will not change in the horizontal. Our antennas not only are above a finite ground
plane but they are located at the edge of the plane. I am sure that both of these
factors contribute to differences in the azimuth distributions of the elevation A
and B patterns.

DR. MILLMAN. I think this is a question of geometry. I am concerned with
any method using amplitude ratios. The locus of all points of equal amplitude ratio
defines a plane that, for example, crosses the center of the main beam at 45

Table 2. Meteor Trail Wind and Height Information

Date	Time	Tape	Range	AZ	EL			Wind		Height	Record
16DEC65	09.9	TAP10	127	330	348	TS	-	V+		73	7
16DEC65	10.2	TAP10	118	330	404	TS	+ 22	V-	29	77	12
16DEC65	10.3	TAP10	160	240	392	TS	-	U-		102	13
16DEC65	10.4	TAP10	144	240	378	TS	+	U+		89	15
16DEC65	10.5	TAP10	178	240	297	TS	-	U-		90	16
16DEC65	10.51	TAP10	168	240	307	TS	+	U+		88	18
16DEC65	10.9	TAP10	92	330	505	TS	+	V-		71	31
16DEC65	10.91	TAP10	112	330	467	TS	-	V+		82	32
16DEC65	10.92	TAP10	122	330	488	TS	-	V+		93	33
16DEC65	11.0	TAP10	125	330	352	TS	+	V-		73	34
16DEC65	13.0	TAP10	112	330	429	TS	+	V-		77	55
16DEC65	14.2	TAP10	116	330	358	TS	- 57	V+	70	69	86
16DEC65	14.6	TAP10	152	240	328	TS	+	U+		84	94
16DEC65	14.61	TAP10	124	240	462	TS	-128	U-	185	90	95
16DEC65	15.5	TAP10	157	240	411	TS	+ 11	U+	14	104	102
16DEC65	16.5	TAP10	121	240	477	TS	-	U-		90	110
16DEC65	17.5	TAP10	121	240	512	TS	+	U+		95	120
16DECC5	17.7	TAP10	98	240	543	TS	-	U-		80	122
16DEC65	18.1	TAP10	145	330	352	TS	-	V+		85	128
16DEC65	19.2	TAP10	117	330	475	TS	- 20	V+	29	36	134
16DEC65	19.3	TAP10	139	330	397	TS	+	V-		90	137

degrees, but as you go off 35 or 45 degrees from the center of the beam you don't know whether the locus is off plus or minus 30 or 50 degrees. The angle of elevation varies quite remarkably.

DR. BARNES. This is one of the reasons that we are now convinced that azimuth angle must be part of this particular system. The only other v that I know of is to go to a very narrow beam, and then you run into other trou s because you won't get enough trails throughout the day.

DR. MILLMAN. This azimuth is also going to affect the height determination from your range.

DR. BARNES. That is correct. That is one saving grace even though the antenna patterns are a hundred degrees wide. Since the signal has to go out and come back and the transmitted beam is narrower, you do effectively reduce the azimuth width of the system. Azimuth determination is one of the things that we have to put into our system. Fortunately, there is room to put it into the system, because we are going to eliminate one of the receivers for other reasons.

DR. MILLMAN. I just wondered the means by which some of the high wind speeds arose in your data reduction.

DR. BARNES. If you look at some of the values, they go as high as 400 for the radial speed. This is something that I will have to write into the program to eliminate, because they don't exist. When I go back and look at them, they look like noise.

THE FLOOR. How are you going to establish some sort of criteria for deciding which ones you throw out and which ones you don't? Where do you decide which to accept?

DR. BARNES. This is something that those of us who have worked in the field have developed. I now have to transfer the information that I have obtained, and that which I will obtain from other people, to the computers. It is a process of teaching the computer, and a computer is pretty dumb. It is easier to teach girls to edit the data. I have been able to show them what the good trails look like on the film and they more or less pick it up. The computer doesn't learn this. So, it is a process of programming the computer. I am going through that stage right now.

DR. ROPER. If you look at something visually, it is surprising the number of oits that are transferred in very small fractions of seconds. You can integrate well into the noise by eye, and it is very, very hard to develop a program which will do this. I think you just need a quiet site. We recommend it to everybody.

DR. BARNES. I appreciate that. One of the objectives that we set for ourselves was to be able to work in noisy sites. We found out that it has gone way over our heads now, but we have developed a system to work with the noise. For instance, with the wind data, the data switches from ones to zeros. The switch is not always a sharp one. What I do is throw out any isolated zeros or ones. That

has improved the data quite a bit. Probably I should require in these cases that there be at least two zeros in a row or two ones in a row.

MR. MYERS. It is really your best bet to purify your answer to more selective information on the input data. You are actually looking at each pulse when it comes in and deciding whether or not this is a reasonable pulse or not, rather than waiting to look at it totally.

DR. BARNES. With the Doppler you have to look at the totality.

MR. MYERS. Yes, but for the rest of the information.

DR. BARNES. Yes. This is true. As a matter of fact, right now, I require that the range be within two counts of the first range that I get. If it is not, then I do not use any of the information that comes in for that pulse. I carry the Doppler forward. I do not compute the elevation angle from the information that is obtained on that particular pulse, and when I get to the place where I have something like four or five ranges that have fallen outside of these counts, that is considered the end of the trail and I don't compute anything else. This can be done very rapidly because, as I mentioned, I took this tape in Tuesday morning and I had it at lunchtime. Now I have to program for the noise. Are there any more questions? Is Dick Southworth here? You have been working with this problem of data reduction; have you any comments or suggestions?

DR. SOUTHWORTH. I haven't looked that hard at your program.

DR. ELFORD. I appreciate your courage in stepping out into the field of working in a noisy site. I don't quite understand the philosophy of it. It seems to me there is a fairly large area of room in this country to set up equipment.

DR. BARNES. The first reason for doing it was as an education for us. We decided that the site initially was quiet enough for performing the experiments, and it was. If we look at the data that came from August, 1964, we see that we have very good data; but the noise has gone up to the place where now instead of getting on the order of 30 usable winds per hour, as in 1964, I am getting five or six. I think that we have learned something. We plan to move this set. We are going to move it up to Durham, New Hampshire, if everything goes right. I have gone up and surveyed, and it is a fairly good site. Any other questions?

Richard Southworth from the Smithsonian Astrophysical Observatory will talk about the Havana, Illinois system.

VI. The Havana System*

Dr. Richard Southworth
Smithsonian Astrophysical Observatory
Cambridge, Massachusetts

DR. SOUTHWORTH. I thought I was going to have some actual data. I am apologetic. We are improving this system, but not as fast as we thought we were going to improve it. We are trying to make it the sort of system that we think it ought to be. In particular, we are trying to get redundant data wherever possible to check our own results. That and just the sheer size of the thing have been a considerable data processing bottleneck. We used to use a film reader 18 hours a day, six days a week, and now we have five to ten times as much data that has to go on tape. That means that the programming for the output is a good deal more complicated. The meteor program has to take the amplitude of each pulse and find the Fresnel pattern in it, and this isn't quite done.

I thought of some special hand reductions, but it was a very long business, and I wasn't sure that I would get the right ones to show you the right answers.

Figure 1 shows the system that we set up, the six stations that were set up for the radar observation of meteors. We are looking at meteors out around where the arrows converge. For measuring winds, we added phase coherence to the system and started to measure the return phase as well as the return amplitude, and we added these two stations (7 and 8) to give us a hold on winds not only out, but also across the beam.

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*(The editor has attempted to reconstruct this presentation from the steno-graphic records and some figures obtained from Dr. Southworth.)

These have Yagi antennas. Actually, they are closer. Their position has to be a compromise between the rate of meteors we will get at this antenna in common with these, and the angle we can put it at in order to get a cross vector out this far at 40 or 50 km. We are going to get roughly 10 percent of the meteors, which would be seen at (8). Another 10 percent at (7). I would have liked a bigger angle.

To define height (Figure 2) we measured the range from the transmitter set directly, which is quite adequate accuracy for that purpose. It is about 150 meters now. To get direction we have two approaches; one is to find the gradient of the direction of the meteor in space by finding the times between the reflection points. The other one would be by measuring for differential ranges and also a loop time to another station and back.

Next slide. (This slide not available, but see Figures 4 and 5 in Dr. Grossi's paper on the first day of the Workshop.) This is one of the film records and we had only six stations. Nice simple clear films to look at. We have 18 channels stacked on top of each other. It is just a matter of the time between successive Fresnel patterns, and by the time you analyze it carefully, you get very high accuracy. The uncertainty in the process of finding the gradient comes much more from finding the velocity. If we can go back to the preceding slide (Figure 2), the problem is we get times between the crossing points quite easily. We have to find the velocity in between, in order to get this distance, and then you put the projection of this distance on these station gradients. It turns out that the error in height is just the proportional error in velocity. This is 1 or 2 percent, 3 sometimes; meaning roughly 1 or 2 or sometimes 3 km of error in height from that difficulty.

One other feature which now works but we hadn't had in the past was simply to get one further coordinate. This told us, of course, what direction the meteor was going, and the distance from the transmitter site must lie somewhere in between, lying somewhere on the cylinder. We didn't necessarily know where. We now measure the phase between the two halves of the double trough at the main site. This will give us the position on this cylinder with far more accuracy than we need, so there will be no error in height due to that.

The big difficulties in height by this approach will be simply the wind effect in these trails. This was analyzed a long time ago in England, and it turns out that one gets, as a matter of fact, about a 3 degree uncertainty. We have analyzed our meteors so we have more than three reflection points where one can see the scatter of times of crossing from those predicted from the single one. It turns out that we get just about a mean error of 3 degrees. In bad cases it could be a good deal larger. Three degrees, of course, means 5 km in height, approximately, and that wind shear with 2 or 3 degrees, or 4 km in height, is the
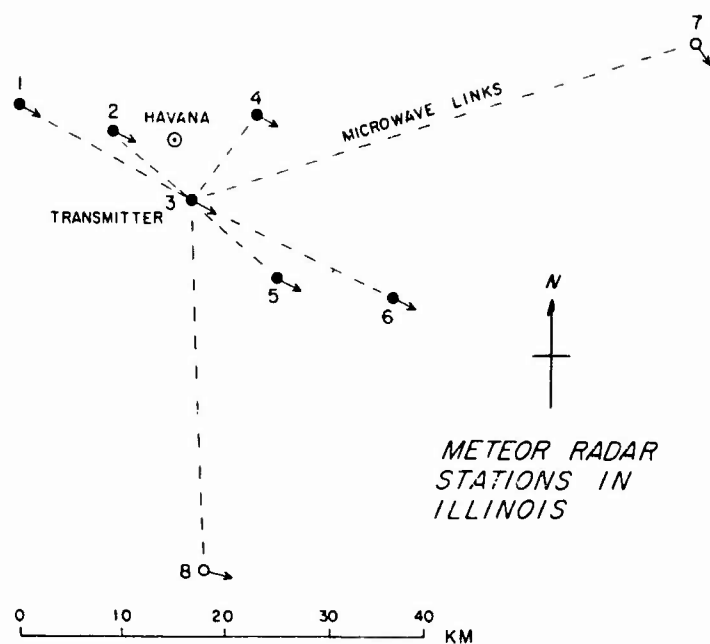
METEOR RADAR
STATIONS IN
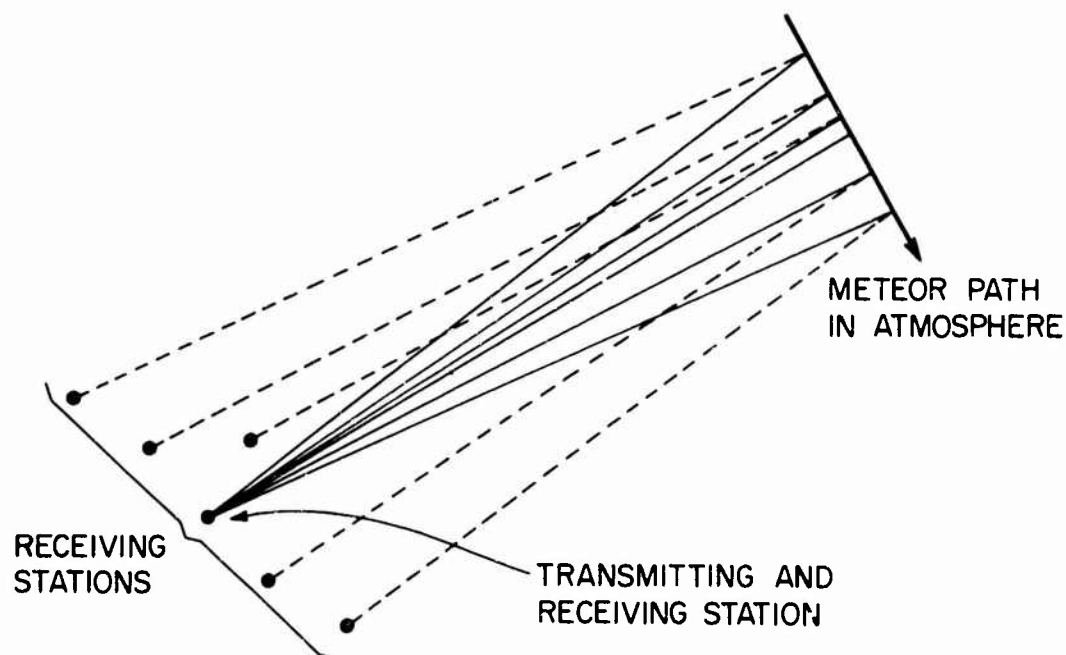ILLINOIS

Figure 1.

Figure 2. Harvard Radio Meteor Project

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controlling error in finding the height by this method if you have not corrected the wind shear. It will be one of the velocity uncertainties.

We will also check this height by the time. Here the uncertainty of 150 meters is roughly set by the fact that we have a one megacycle clock count and our range is just to accommodate the cycles to the clock. Dr. Schaffner, who devised the design of the electronic equipment, I am sure, could make a much faster system. Just as long as we end up at the leading edge. I don't know that we are going to push this a great ways down.
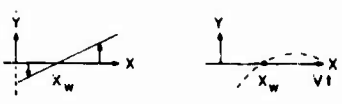
Working out from spacing here, it is roughly 11 km that we have between the two adjoining stations, and 200 km range here. It works out to an uncertainty in height of 6 km, if one is to find it that way, but if I use a larger range of stations, I can get this down to about two. If I use a larger one way out to the outside, it will be down to one. That method is therefore comparable in accuracy with the one involving the gradient correcting for wind errors and the two will check each other. We come out with 1 or 2 km uncertainty in height. I almost wish now that we had measured the phase between the halves of the antenna, but I really don't want to change those antennas. They are very good antennas, but they are also very big.

Now, I would like to make a few points on correction for wind shears, (Figure 3). If I take the coordinate X measured along the trail of a meteor, Y measured normal to it, and assume the radial wind $\dot{Y}$, the motion of the ion trail is a linear function of the shear times position minus the crossover point, so that the trail itself is the head of the meteor and the trail itself is the parabola, and for general convenience, I use a parameter alpha rather than the shear A, which is much simpler. It has long been known that you get apparent meteor velocity from the Fresnel patterns, which is a function of the true velocity, and this is not a very large error unless alpha comes close to plus one half. The normal order for alpha is plus or minus one, however it seems that with the slow meteors I can actually do this. I can actually curve the trail to the same curvature as the front of the wave from the radar.

If now I measure time from the apparent cross-over point on the Fresnel pattern, and subtract out the Fresnel phase, I will show you how, somewhat like the empirical Fresnel pattern, I come out with the shift in phase due to wind.

Now, I observed this parabola in phase, and I can find from that, without any ambiguity, any correction necessary for velocity and correction necessary for the time in passing of the theoretical reflection point. I note, incidentally, the actual point of tangency. I also note the length of the Fresnel zone is alpha. So, the amplitude of the signal is considerably changed.

The embarrassing part of the whole thing is that I do not have an unambiguous answer to the wind. I find for my parameters an A and X over W. Thus, the rate of change of the wind along the trail is not the observed rate of change of the

RADIAL WIND $\dot{Y} = a(X - X_w)$

$a = \sigma R/v$

APPARENT VELOCITY $v' = v(1-a)/\sqrt{1-2a}$

APPARENT TIME $\delta t = -a\, X_w/(1-a)v$

$t' = t - \delta t$

OBSERVABLE WIND PHASE $\phi = \dfrac{-2\pi}{\lambda R}\left(\dfrac{a}{1-a}\right)^2 (v't')^2$

$\qquad - \dfrac{4\pi}{\lambda R}\left[1-\left(\dfrac{a}{1-a}\right)^2\right]\dfrac{a X_w}{\sqrt{1-2a}}\, v't'$

$\qquad + \text{CONSTANT}$

CORRECTIONS $\dfrac{v}{v'} = \sqrt{1-\left(\dfrac{a}{1-a}\right)^2}$ $\qquad \delta t = \dfrac{-1}{v}\dfrac{a X_w}{\sqrt{1-2a}}$

TANGENT POINT OF WAVE AND COLUMN $\quad X_0 = -\left(\dfrac{a}{1-2a}\right)(X_w + vt)$

$\qquad Y_0 = (a-1)X_0^2/R - a X_w vt/R$

FRESNEL ZONE $\quad F = \sqrt{\dfrac{R\lambda}{1-2a}}$

WIND ANSWERS $\quad \sigma = \pm v\left|\dfrac{a}{1-a}\right|/R\left(1\pm\left|\dfrac{a}{1-a}\right|\right) \qquad X_w = \pm\dfrac{v}{v'}\dfrac{a X_w}{\sqrt{1-2a}}/\left|\dfrac{a}{1-a}\right|$

Figure 3.

Doppler velocity. It comes pretty close when alpha is pretty small, and it is not even plus or minus the same thing, and this has equal uncertainties. However, these two lines do have one point in common. That happens to be the observed velocity at the apparent specular reflection point. Therefore, I take that point and I have to solve the ambiguity of the wind shear by other methods.

One thing I will have to do before I attempt to do anything else, I will simply see to it that this term is of the proper sign. If it is not, then I am quite sure that I may not apply this form. I can't assume this wind shear. If I can't assume this wind shear, then I am not going to try to analyze that record.

1 will come back to solving the ambiguity in a little bit. Right now, I would like to show you how I measure the wind velocities (Figure 4). This figure shows an ordinary meteor. This spiral B is from the meteor decay. This is very confusing. The wind takes this whole pattern and spins it around and around. I want to find out the rate of the spin. My process is going to be to subtract the Fresnel variations from the rest of it, to leave the point where this nondecaying turbulence moves around in a spiral. The empirical analysis is to leave a straight line and work around it. I have two tracts of records, one gives, for every pulse, the logarithm of the amplitude to the received signal; and the other one gives the alternate pulse c *· linear scale, the amplitude times the sine, and the amplitude times the cosine

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of the phase. The first thing I do with this record is to interpolate it so that I have it at every pulse.

Then I have a rough picture of what is going on here. I treat the amplitude record C as we have always done in the past. We loo¹ at the phase of the maxima, after giving us a rough idea of what the wind is doing. I can then subtract and look at the minima. This treatment will also be done later for the diffusion. Here I interpolate to the maxima and to the minima and the intermediate points and find the mean point. Well, that gives me the size of the loop and I solve again for the wind velocity where wind phase is the parabola in time. I do it over again. About twice gets it right. That will give me the coefficients I needed for the other proc-ess.

Now, first of all, I have been trying to get from this system two coordinates of the wind. For that, I have the movement of the trail, I have the reflection point say from one station to along our general line giving me the wind, and I also have at each point a slope with ambiguity. I may point out also that normally I expected to look ahead, because we have the spacing along the trail which is of the order of 2 or 3 km. If there were any large irregularities on the wind, I have smeared them out already. So, I expect a well determined curve.

If there were in fact large variations, I have one other means of treating it. This is the matter of measuring the diffusion range.

Now, the great difficulty with that system is that I started with a reflection point here on the trail, where I had so many ions per meter, and my reflection point moves along to where I have more ions per meter. So, I had no reason to ex-pect that in the first instance there would be a rate of change; that is purely confus-ion. It has a lot to do with the ion moving along the trail. If I assume that that is true, then I can correct for it, and we are doing our best to find out what these curves are. On every little wobble on the Fresnel pattern, we compute the number of electrons per meter at any one reflection point on the trail.

Right now, our system of amplitude calibration graphs doesn't tell me very much. I can find out a lot from position in time, Fresnel maxima, and my ampli-tude measurements are not what I wish they were. Assuming that I had a trail there, then I can make the appropriate correction to this wind shear using one or the other corresponding to the meteor direction along the trail. I can measure one direction for the other for a diffusion to come out with a reasonable or unreasonable answer. I will try to tell you what some of the other patterns are.

(At this point the stenographic record became uninterpretable.)

DR. BARNES. Yes, but won't the other stations give you wind vectors in other directions, Dick?

DR. SOUTHWORTH. Yes. But I want two vectors at the same point on the trail.

DR. BARNES. But you get that, don't you?

DR. SOUTHWORTH. Yes, occasionally I will. It may be that I end up supplying AFCRL with only a few cases of meteors with coincidence. Then, of course, the other factor is simply the angle between the two wind vectors. It depends upon the spacing of the stations.

Now, if I want a third component, as Dr. Grossi mentioned, I cannot do this with any single meteor because no trail will scatter to one station that will also be observable here and here. The trails are not long enough. Well, I wouldn't get reflection at the same point anyway. So, I have to have two meteors coming through at about the same point in space at the same time.

Maybe I can show you what we intend to do by a brief discussion about how we are generally thinking of treating the sort of wind we believe we are going to get. Each of these meteors gives me essentially a wind profile along the trajectory, and I am going to treat, for the time being, individual levels of one kilometer. It is a little closer than our resolution, but it is handy enough (see Figure 5). In the volume we are looking at, each meteor will give me a component; and for the moment transforming everything into horizontal winds over a large number of points and times. I shall propose a grid system, the size to be found when somebody tells me what the wind scale is in the horizontal.

When I have some meteors, say 10 percent will give me the profile at one point, or it will give me a two point profile in the plus component, well, I will simply run them onto some other grid that has fewer points. When I find that there are enough, when I have two points fairly close together here which do not involve the same stations, we are going to get just a bit of vertical component. Then I will solve for a three-dimensional wind in a similar function. We have at least enough redundancy, we are hoping for redundancy, to find vertical winds, ten meters per second or more. We don't see why we shouldn't.

DR. BARNES. Any questions?

MR. NOWAK. It occurred to me that you have one of your outrigger stations at a distance of 40 miles. I assume that you are looking at some fairly mean elevation angles, 45 degrees elevation angle or so?

DR. SOUTHWORTH The antenna was aimed to look at the same volume.

MR. NOWAK. But at what elevation angle do you look in a general way?

DR. SOUTHWORTH. That was roughly 45. The further ones are at 35.

MR. NOWAK. Then you should have the two points you are looking at with these two stations close to the transmitter and the outrigger station at 40 miles should be over 20 miles apart in the typical case, depending upon the trail. Do you really think that you can get very many echoes where you get two points simultaneously from the same trail 20 miles apart?
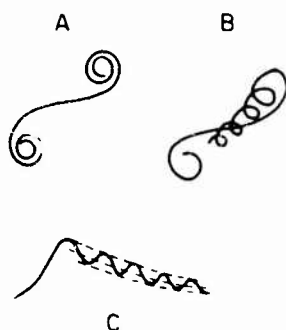
Figure 4.
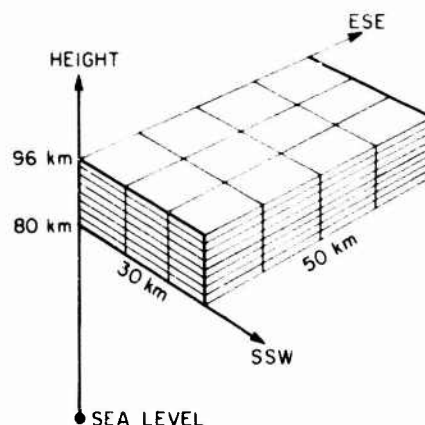
Figure 5.

DR. SOUTHWORTH. I am not even going to consider that. I am never going to do anything with the echo from the outrigger station unless it is bracketed from the main station by echo points. If I get something from out there, in the first place, I am not likely to record it.

MR. NOWAK. So you haven't gotten the orientation?

DR. SOUTHWORTH. Well, you would have to extrapolate, and I am not going to extrapolate.

DR. MILLMAN. What is your estimate of the number of meteors you can get which can be subjected to this type of analysis?

DR. SOUTHWORTH. We thought we were going to get 500 an hour when the systems were working efficiently, which we have not reached. How many we need depends upon the size of the horizontal cell. We were guessing at 20 or 30 km. I was also guessing 20 minutes for a time.

DR. MILLMAN. And you have this system, or you will have it worked into a computer system?

DR. SOUTHWORTH. Yes, each meteor is produced on tape, and it has a card output, card or tape is fed into it for this treatment. We are not really trying to run in real time.

DR. MILLMAN. In the process of examining and selecting, I take it, this is your outline you have?

DR. SOUTHWORTH. Yes. Any meteor that got through the analysis procedure, which has a very large number of checks in it, could be put into it. In any case, it has a high probability of error in it. I am willing to put in something with a very large error probability.

DR. BARNES. Al Cole and I discussed the size of the cells that you should be using. If you want to look at gravity waves, you will have to use smaller cells or a different approach to the problem. You plan to use cells that are 10 by 10 by 1 km, if I remember correctly.

DR. SOUTHWORTH. Yes. (See Figure 5.)

DR. BARNES. This is what you are currently going to look at. Now, perhaps for studies of turbulence and studie f gravity waves, we need something smaller. If we are going to look at the diurnal variations or the synoptic variations, we could probably lump the whole level together, so therefore this is a compromise until we find out a little more about motions at these levels.

DR. SOUTHWORTH. If you can show me some gravity waves, then I can run an analysis on them.

THE FLOOR. With these high wind shears, the point of reflection moving along the trail and the ion densities changing, to what extent would this affect the height determination?

DR. SOUTHWORTH. Well, that is what I am going to talk about tomorrow.

DR. ELFORD. You range at present to one receiving station?

DR. SOUTHWORTH. There are eight receiving stations.

DR. ELFORD. And you range to each of them?

DR. SOUTHWORTH. We can. That equipment is now installed. We have not finished any reduction of any meteors.

THE FLOOR. So it is correct then that you obtained the orientation from the Fresnel diffraction patterns and the position on the trails in space by means of the various stations?

DR. SOUTHWORTH. I find it two ways. Partly by the spacing of the Fresnel pattern which gives the gradient.

THE FLOOR. This just gives you the orientation of the trail. It doesn't tell you where it is.

DR. SOUTHWORTH. Okay. I could have then found it purely from ranges without any of the Fresnel diffraction pattern or the other. I will also find it by the diffraction pattern by comparing the phase as received from two halves of the antenna at the main site.

THE FLOOR. I think there is redundancy there.

DR. SOUTHWORTH. Yes, we are delighted to have it. Incidentally, in this case we will be able to measure between these two halves very accurately, because I will have measurements at every pulse, angles at every pulse for a large number of pulses to receive this variation.

VII. The French System*

Dr. Isaac Revch
CNET, France

DR. REVAH. From these parameters (elevation, azimuth, distance, the phase of the reflected wave as a function of time), we compute by an electronic computer the height of the point of reflection of the trail, the horizontal coordinates of this point of reflection of the trail, and the wind velocity of this same point.

I pointed out yesterday that theoretically we are able to achieve a localization of the order of 500 meters. Our first measurement had an accuracy which was smaller than this, and the results of September, November, and December (which I am going to present to you) had an accuracy which was near plus or minus one kilometer on the phase measurement and on the horizontal coordinate.

The number of echoes we obtained in half-hour periods during the period of 7 to 9 September, for 24 hours of recordings, have a maximum about ten o'clock in the morning of between 600 to 800 echoes per hour. These were echoes with amplitude greater than-115 dbm, and we in fact use for our present measurement only echoes the amplitude of which are greater than minus 100 dbm.

In fact, Figure 1 shows a more realistic view of the wind measurement. We have here as a function of time and height for November 16 and 17, 1965, the

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*(Dr. Revah consolidated his remarks into one extended abstract which has been placed with the other papers presented on the first day of the Workshop, page 83.

Since some of the material covered by Dr. Revah does not appear in detail in the abstract, the editor has taken the liberty to include some parts of Dr. Revah's talks as they were recorded by the stenographer.)
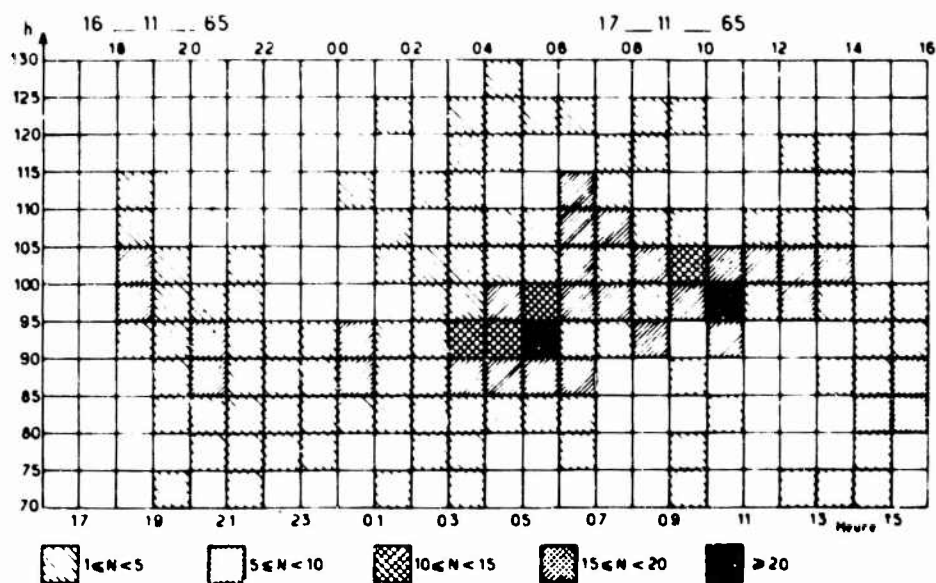
Nombre d'échos météoriques N en fonction de l'altitude h et de l'heure
RADAR METEORIQUE (GARCHY)

Figure 1.

number of usable echoes we obtained on which we were able to make our wind
measurement.

The maximum rate we had for a 5 km, one hour box was greater than 20 per
hour. The total amount of data for the 48 hour period was only 1000 echoes. This
is smaller than the several hundred echoes observed per hour because we are lim-
ited in the data reduction method.

DR. ELFORD. Could we just refer to this Figure while you have it there? We
are talking about the variations in height as a function of time, and this shows it
very much.

DR. BARNES. I am glad to see independent support of this variation of height
with time of day. As I mentioned to some of you before, this variation is another
very strong reason for obtaining the height of each individual trail.

DR. REVAH. The height goes from 70 to 130 km. At this time we were not
able to measure echoes higher than 115 km accurately. In fact, above 115 km
there are less than five echoes per hour.

The region covered by the system is 80 km in the north-south direction, and
200 km in the east-west direction.

We have made an analysis of the hourly mean values of the wind velocity, and
for the 12-hour component the results were in complete agreement with those

obtained by Greenhow. That means that we have an amplitude and a phase that is increasing with height. The other components which we studied were the 24-hour component, the 8-hour component, and the 6-hour component: but the number of samples was not great enough to permit accurate determinations, so that we were only able to compute the 12-hour component of the hourly mean values of the winds. You can see that in fact all the things here are preliminary results which were recorded during September, November, and December. In fact, we are trying to have a record every month for consecutive time at certain hours of the day. That means that we can record two or three days continuously per month. At the present time we have 400 hours of records and only 90 hours of records have been analyzed. That is due to the fact that it takes a long time to read the parameters from the records. That is why we are waiting for our tape recorder, and we shall then perhaps be able to be a little faster in our computations.

In conclusion, our first wind measurements have confirmed the downward motion of the wind profiles during night hours, and have extended these results to day periods. We also intend to make analysis of the tidal components, since we have extended periods of observation for a year, and we think that the amount of data that will be available will be enough to establish accuracy for both tidal components.

The analysis of several other parameters that need good accuracy and height measurement will be made also in the future, and in fact, we shall look first of all at the small slope of the equiphase wind surfaces. This small slope has been predicted by the gravity wave theory. We hope it makes possible the measurement of this small slope of the equiphase wind system. As regards the density measurement at high altitudes by the echo decay method, we have found on our first results a strong correlation between the heights we measured directly, by measuring the angles of arrival in the distance, and the heights we can deduce from the decay of the signal.

Last, we are also planning, and we have already started, a comparison of wind measurements with sporadic E as observed by an observer looking at the same region of space as the one that we can see.

PROFESSOR PETERSON. I wonder if you could comment on the calibration of your equipment?

DR. REVAH. We have made calibration tests using an airborne transmitter, helicopter, and balloon. It was only a calibration of the angle and distance measurement. It was not a calibration of the diagram of the antennas. It was made for the purpose of looking at our calibration method to see if it was good enough. In fact, the accuracy I gave you on the angle measurements was .2 degree for the angle measurement with a good signal to noise ratio, and 300 meters for a distance measurement. Those calibration tests have shown at the present time the measurement is not accurate enough and it is in fact ± 1.5 degrees in elevation, and the result is

not known theoretically. In a good portion of our diagrams, it is on the order of 1 km, but as we go higher in elevation, the accuracy is not the same. It is going nearer to the theoretical one. We have plans for the future, that means the beginning of September, to have our antennas diagrammed, i.e., calibrated in amplitude, and we shall also use high altitude balloons going to heights near 30 km for our precise calibration, so that we shall be able to look with greater accuracy at the occurrence of this whole thing. The difficulty with that is that we are in the center of France, and we have many planes flying overhead, and it is not easy to have balloons lifted, you see.

DR. SOUTHWORTH. Did you say 40 km for the balloons?

DR. REVAH. No, 30.

DR. SOUTHWORTH. Even so, how do you put a balloon at that height in your beam?

DR. REVAH. It wasn't done exactly for the effect of putting it exactly in the beam. It is not intended to look at the diagram of our beams. This balloon will be used especially for the distance calibration. In fact, our distance calibration is easy to do using the low altitude transmitters, due to the fact that we have to be at the common region of the transmitting and receiving antennas so that we shall try the angle of calibration at heights that are smaller than those where the meteors appear. If I remember what I said yesterday, our transmitting beam is narrower than our receiving antenna, so that the thing that we have to do is be sure that we are in the transmitting beam and even if we are at the lower part of our receiving antenna beam, we might be able to calibrate the distance better than by using a helicopter or balloon.

DR. MILLMAN. How do you intend to locate your balloons in space at 30 km?

DR. REVAH. It is a matter of meteorological work. We hope to obtain balloons with transponders, and we shall use pulse equipment at the same station. It is a very difficult thing to do, because we do not have this equipment. It is held by the meteorologists, you see.

DR. MILLMAN. Well, the meteorologists aren't always happy with the quality of their equipment, either.

DR. REVAH. Yes, I know it. In fact, the trouble we have with our records is the number of planes flying between our transmitting and receiving antennas, and we have more plane echoes in an hour than meteor echoes.

THE FLOOR. A plane echo can be used on cw to calibrate the direction.

DR. REVAH. We have calibrated the zero distance of our measurement using the planes, and we have also calibrated the receiving and rotating antenna; it was easier than by other methods.

DR. ROPER. One interesting thing I would like to point out -- the 24-hour component is indicative of a wave length of about 24 km, which is exactly what we found at Adelaide. The 12-hour component is much, much longe. , which is just as it should be.

VIII. Upper Atmospheric Wind Parameters from Radio—Meteors*

Prof. T. R. Kaiser
University of Sheffield
Sheffield, England

Abstract

Information concerning the wind structure in the meteor region can be obtained from measurements of the Doppler shift in radio waves reflected from meteor trains and from the fading of enduring meteor echoes due to train distortion. It is shown that the wind shear can be deduced from the time variation in the Doppler shift. Particular attention is given to fading observations since they are much simpler to make than the Doppler measurements, requiring much less sophisticated apparatus. It is shown that measurements of the time delay, before the onset of fading and of the initial fading period, should yield useful information concerning the magnitude and vertical scale of the wind structure.

1. DOPPLER MEASUREMENTS OF WIND AND WIND SHEAR

The Doppler shift in the radio echo received from a meteor train gives a measure of the line of sight translation of the train; at meteor heights this will be due

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*(Read by H.G. Müller on 17th August 1966)

to the neutral wind. This technique has been used successfully by workers at Jodrell Bank (England), Adelaide (Australia), and elsewhere. If simultaneous measurements are made with two low elevation aerial beams at right angles, it is possible to obtain two components of the mean wind vector (assumed to be substantially horizontal) and the mean wind shear can be deduced, if a height finding method is employed. Alternatively, the heights of the meteor reflections can be obtained from the duration of decay-type echoes using an experimentally determined relation between duration and height. Multi-station observations, which can be made with a single transmitter and spaced receivers, permit simultaneous wind observations to be made at points along a train spaced by several kilometers. Since the train orientation can also be obtained from the time displacement of the echo patterns, this technique is particularly useful for studying the instantaneous vertical wind profile.

It has been shown (Kaiser, 1955) that the presence of a vertical wind shear will cause the Doppler shift to vary with time, but as far as is known this has not been utilized elsewhere to derive the wind shear from individual meteor echoes. The apparent radial drift velocity u_a, in the presence of a constant wind shear, is

$$u_a = \frac{(2 - \delta)}{2(1-\delta)} u_o - \frac{u'^2 R_o t}{1 - \delta}, \tag{1}$$

where $\delta = 2u'R_o/v$; u' is the gradient of the radial component of the wind velocity measured along the train; R_o is the radar range to the initial specular reflection point O (Figure 1) where the radial component of the wind is u_o; v is the meteor velocity and t is the time taken as zero at the instant the meteoroid is at O. Generally δ is small compared with unity and (1) simplifies to

$$u_a \simeq u_o - u'^2 R_o t. \tag{2}$$

Equation (2) is easily derived from geometrical consideration of the movement of the specular reflection point along a train subject to a wind shear. It means that the shear adds a component to the apparent drift which is always toward the observer (independent of the sign of u') and which increases with time. Thus from measurements of the rate of change of the Doppler shift, the magnitude (but not the sign) of u' can be deduced and hence information about the instantaneous wind shear can be obtained from a single meteor echo.

If the wind is assumed to be horizontal, then its component in the azimuth from the observer to O is $u_o(\cos\theta)^{-1}$ and the vertical gradient of this component is $u'(\cos\theta\cos x)^{-1}$, where θ is the elevation to O and x is the zenith angle of the train.
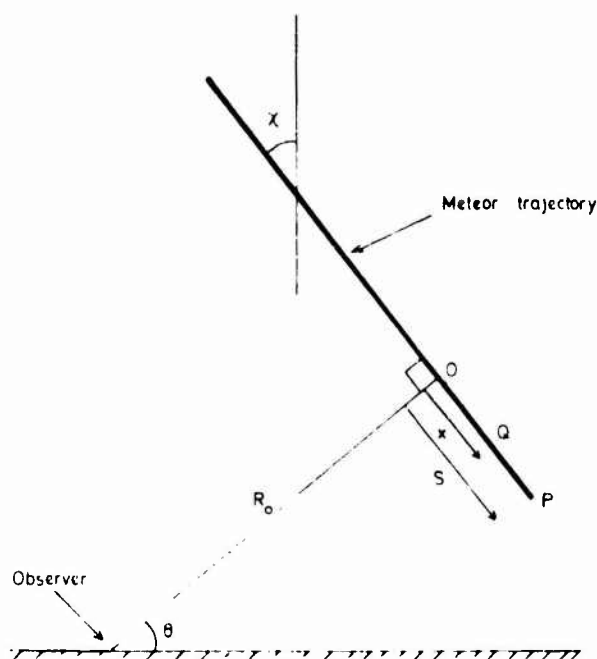
Figure 1. The Instantaneous Position of the
Meteoroid is P (the meteor trajectory is not
necessarily in the plane of the diagram)

Rao (1958) has analysed Doppler records taken during the Geminids (1948) and
has observed linear variations of Doppler frequency with time. He has explained
this as due to the effective reflecting point moving towards the maximum echo dura-
tion level for the particular train. This is clearly a misinterpretation since Rao
states: "If the result of the combination of echoes from the various sections of the
train may be visualized as equivalent to an echo from the effective point of reflec-
tion ... then the aforesaid effect may be transformed into that of the drifting of this
effective point of reflection towards the level of maximum echo duration along the
meteor train." Thus he appears to visualize multiple reflections with those origi-
nating near the level of maximum duration persisting longest. It is clear, however,
that multiple reflections cannot give a single instantaneous Doppler frequency and
are therefore inconsistent with the linear variations observed for these echoes. On
the other hand, if we apply Eq. (2) to Rao's data (for 8 Geminid echoes), we obtain
an average value for u' of approximately $3 \times 10^{-3} \sec^{-1}$. This is a wind shear of
3 m $\sec^{-1}$ per km and is of the expected order of magnitude. Rao's value for the
mean wind is 54 m $\sec^{-1}$ and is of the same order as found by other workers.

Measurements of wind shear have been made in this way at Sheffield during
the IQSY and will be described by H. G. Müller in another paper.

2. THE FADING OF ENDURING METEOR ECHOES

It is widely recognized (Greenhow, 1952; Manning, 1959) that the fading of enduring meteor echoes is due, primarily, to distortion of the trains causing additional perpendicular reflection points, each giving rise to what Manning refers to as a 'glint.'

The location of a glint at a given instant of time can be determined by applying the stationary phase condition as follows:

Let $u(x)$ be the radial component of drift velocity at the point Q, distance x from 0 (Figure 1). To a sufficient approximation, the relative phase of the signal returned from Q is

$$\phi(x) = \frac{2\pi}{\lambda}\left[\frac{x^2}{R_o} + \frac{2(s-x)u(x)}{v}\right]. \tag{3}$$

where λ is the radio wavelength. The reflection points are thus located wherever $\phi'(x) = 0$, i.e., at values of x satisfying

$$\frac{x}{R_o} + tu' - \frac{xu' + u}{v} = 0, \tag{4}$$

subject to $x < s$. The meteoroid is instantaneously at P (Figure 1) where $OP = s$. Provided that u and u' are sufficiently small that the third term on the left-hand side of (4) can be neglected relative to the first two terms (i.e., that the train can be regarded as instantly formed), the reflection condition becomes

$$f(x) = \frac{x}{R_o} + tu'(x) = 0. \tag{5}$$

The zero order reflection commences at $x = 0$, $t = 0$ and subsequently moves along the train from 0. If the third derivative of u with respect to x is nonzero, additional glints will subsequently appear at other values of x satisfying (5). Thus the first glint will arise at time T when $f'(x) = f(x) = 0$ and will immediately split into two glints. These will initially be closely spaced along the train and hence will have approximately the same values of u; they will thus interfere with the zero order reflection (which in general will have a significantly different value of u) so as to produce initially a sinusoidal fading pattern. As further glints appear the fading pattern changes from a sinusoidal to a Rayleigh one.

From the above, we see that the fading commences when the first glint appears at $x = x_1$ after a time T, where x_1 and T satisfy the simultaneous equations

$$x_1 + R_o Tu'(x_1) = 0 \qquad (6)$$

and

$$1 + R_o Tu''(x_1) = 0 . \qquad (7)$$

The zero order solution of (5) for $t = T$ occurs at some value $x = x_o$ and hence the relative drift velocity between the two reflection points, at the onset of fading, is

$$\delta u = u(x_1) - u(x_o) , \qquad (8)$$

and the initial frequency of fading is

$$\nu = \frac{2 |\delta u|}{\lambda}. \qquad (9)$$

For a given wind profile, Eqs. (6), (7), and (8) enable us to calculate the time delay before the onset of fading and the initial fading frequency (or period).

Evidence from observations of enduring visual trains and from rocket vapour release experiments indicates that the wind velocity in the meteor region has a quasi-periodic variation with height, and it is interesting to evaluate the distribution in the values of T and ν to be expected. The simplifying assumption is made that the meteor train retains a detectable line density over a height interval greater than the vertical period of the wind fluctuation. Although it is clearly somewhat artificial, a sinusoidal profile will now be considered, since it will illustrate the main features of the problem.

2.1 Sinusoidal Wind Profile

Let $u = u_1 \cos y + u_o$, $\qquad (10)$

where $y = kx + b$, $k = \frac{2\pi}{a}$; a is the wavelength of the wind fluctuation along the train; and b is a constant (all values of b between $-\pi$ and $+\pi$ are equally probable). The initial location of the first glint, $y = y_1$, thus satisfies [from (6) and (7)]

$$y_1 - \tan y_1 = b \qquad (11)$$

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and occurs at time $T = Z(R_0 k^2 u_1)^{-1}$, where

$$Z = \sec y_1 . \tag{12}$$

The zero order reflection at time T is, from (5) and (10), located at $y = y_0$ where

$$y_0 - Z \sin y_0 = b . \tag{13}$$

Note that y_1 and y_0 satisfy the following conditions:

$$0 < b < \pi , \quad y_1 < 0 , \quad y_0 > 0 ,$$

$$-\pi < b < 0 , \quad y_1 > 0 , \quad y_0 < 0 .$$

From (9) and (10), the initial fading frequency is given by

$$\frac{\nu\lambda}{2u_1} = \left| \cos y_1 - \cos y_0 \right| . \tag{14}$$

Figures 2 and 3 give Z and $\left| \cos y_1 - \cos y_0 \right|$ as functions of b, while Figure 4 shows $\left| \cos y_1 - \cos y_0 \right|$ as a function of Z. The maximum value of $Z = 4.56$ occurs for $b = \pm \pi$ and the minimum value $Z = 1$, for $b = 0$.

Since all values of b between $-\pi$ and π are equally probable, the distribution of Z (and hence in $R_0 T$) can be shown to be

$$N_z \propto \frac{db}{dZ} = (1 - Z^{-2})^{\frac{1}{2}} , \tag{15}$$

where $N_z dZ$ represents the number of values between Z and $Z + dZ$. N_z is given as a function of Z in Figure 5. The mean value of Z for $-\pi < b < \pi$ is $\overline{Z}$, given by

$$\overline{Z} = \frac{\int_1^{4.56} N_z Z \, dZ}{\int_1^{4.56} N_z \, dZ} = \pi^{-1} \int_1^{4.56} (Z^2 - 1)^{\frac{1}{2}} dZ = 2.90. \tag{16}$$
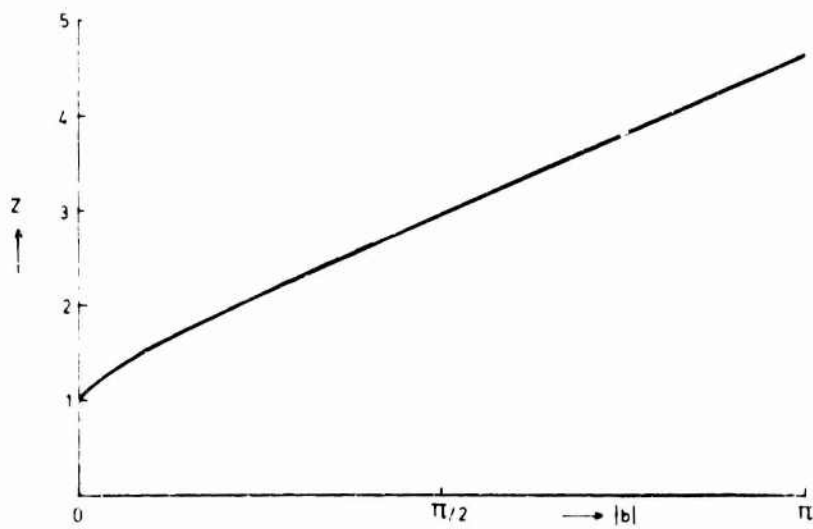
Figure 2. Graph of $Z = R_o k^2 u_1 T$ as a Function of b (sinusoidal wind profile)
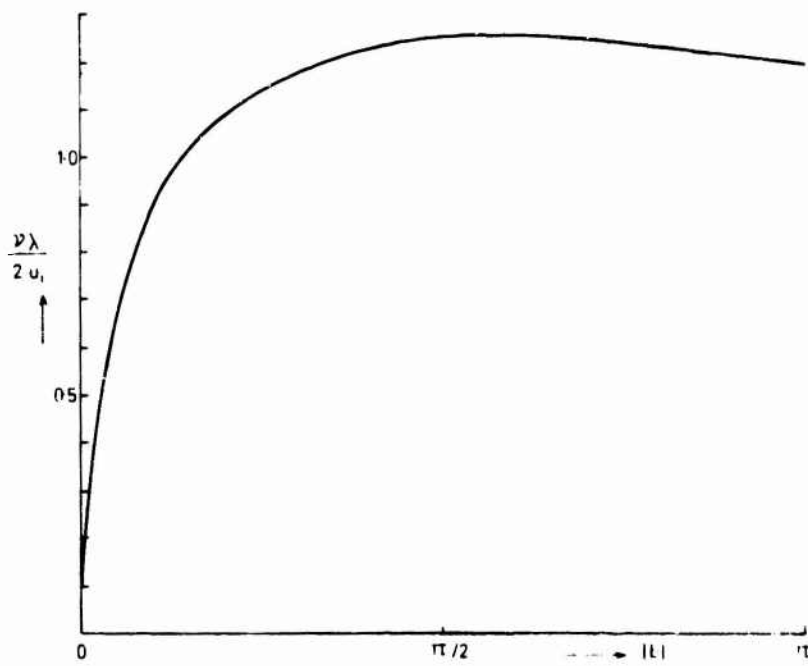

Figure 3. Curve Showing $\dfrac{\nu\lambda}{2u_1} = \left| \cos y_1 - \cos y_o \right|$ as a

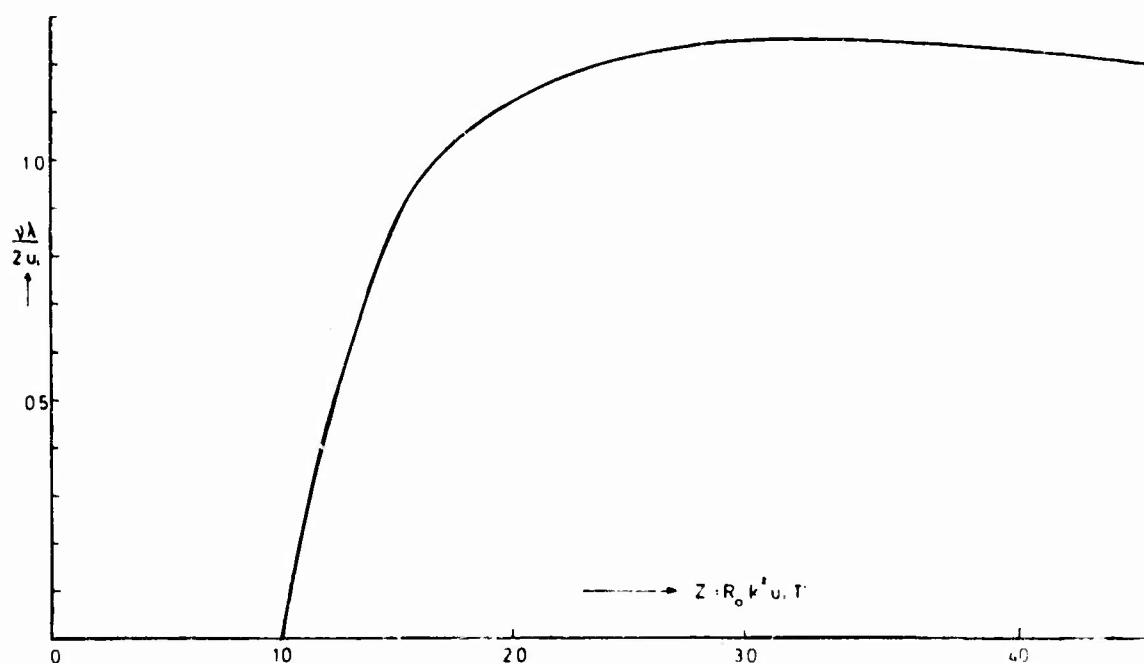Function of b (sinusoidal wind profile)

Figure 4. Graph of $\frac{\nu\lambda}{2u_1}$ as a Function of $Z = R_o k^2 u_1 T$ (Sinusoidal Wind Profile)
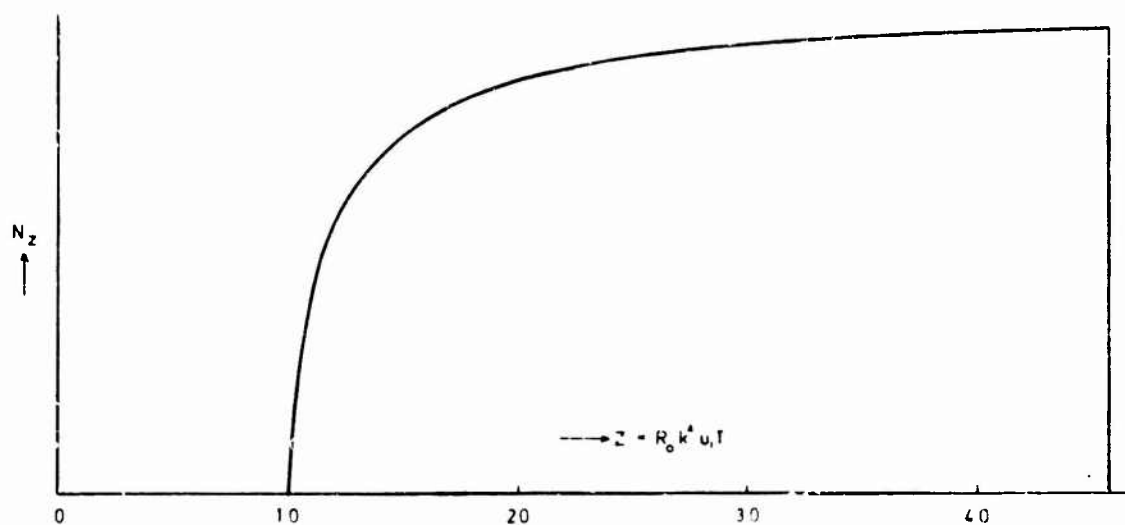

Figure 5. Distribution in Time Delay Before Onset of Fading ($Z = R_o k^2 u_1 T$, Sinusoidal Wind Profile)

Thus the mean value of $R_o T$ is (from 16)

$$\overline{R_o T} = 2.9 \ (k^2 u_1)^{-1}. \tag{17}$$

The mean value of ν for $-\pi < b < \pi$ is $\overline{\nu}$ and can be determined by numerical integration, yielding

$$\overline{\nu} = 2.3 \ u_1/\lambda. \tag{18}$$

The distribution in ν is N_ν where $N_\nu d\nu$ represents the number of values between ν and $\nu + d\nu$. It can be evaluated numerically from Figure 3 and is given in Figure 6.

From measurement of the initial fading frequency the amplitude u_1 of the wind profile can be deduced [using (17)], and hence, from measured time delays T, the spatial period $a = 2\pi/k$ can be found from (17).

A representative value for u_1 is of the order of 50 m sec^{-1}; hence, for a wavelength of 10 m we may expect, from (18), $\overline{\nu} \sim 10$ sec^{-1}. This may be compared with Rao's average (Rao, 1958) of about 8 sec^{-1}. Greenhow (1952) found $T \sim 0.4$ sec; thus for $R_o = 300$ km, $u_1 = 50$ m sec^{-1}, (17) yields $a \simeq 9$ km, which again is in general agreement with other data.

In concluding this section, it must be pointed out that the distribution function and averages have been calculated on the assumption that $u(x)$ is the same for all meteors observed. Clearly, temporal variation in the wind, as well as variation in x and θ between different meteors, will cause some additional spread in the observed distributions of $R_o T$ and ν. It should be noted, however, that for observations at relatively low elevations, both $\cos x$ and $\cos \theta$ will not be very much different from unity for most meteors; hence, this effect should not be large.

2.2 Trapezoidal Wind Profile

The profile considered is illustrated in Figure 7. The limiting case considered is that for which the gradients occupy a train length which is short compared with the spatial wavelength a. It is easy to show that in this case the initial fading frequency always has the same value

$$\nu = 2 \ u_1/\lambda. \tag{19}$$

The theoretical distribution in delay time T is shown as a function of $R_o u_1 a^{-1} T$ in Figure 8, and the mean value of $R_o T$ is
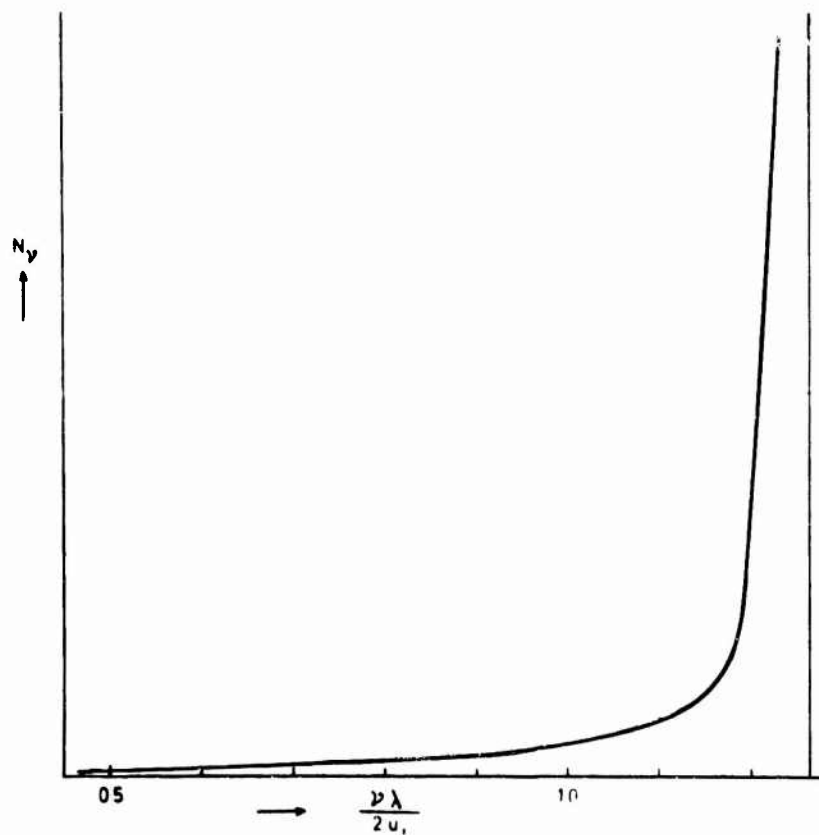
Figure 6. Distribution in Fading Frequency ν as a Function of $\nu\lambda/(2u_1)$ for a Sinusoidal Wind Profile

$$\overline{R_oT} = 0.375 \, a \, u_1'^{-1}. \tag{20}$$

In this case, further information is needed to derive the wavelength a from observation.

It is interesting to note that the initial fading period is clearly not very sensitive to the precise form of the periodic wind profile [Eqs. (18) and (19)]. Also, in the sinusoidal case, if u_1', is taken as the maximum wind gradient, then $ku_1 = u_1'$ and (17) becomes

$$\overline{R_oT} = 0.46 \, au_1'^{-1}. \tag{21}$$
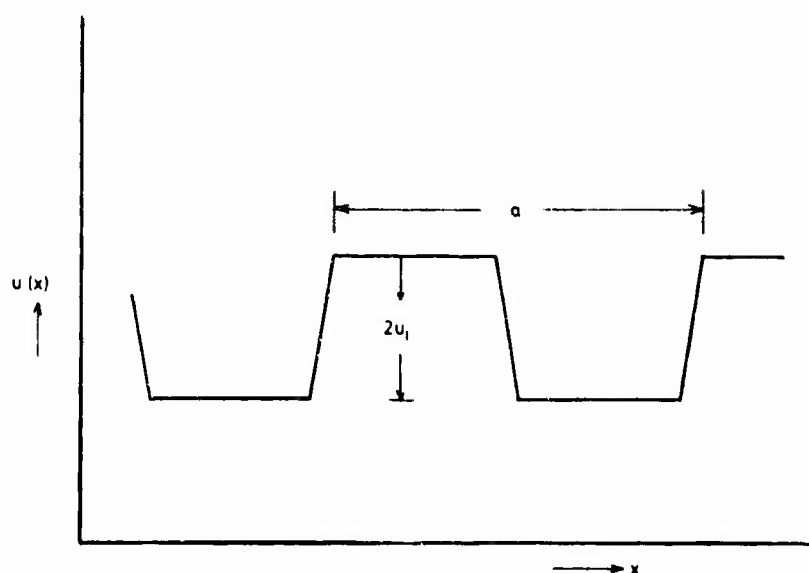
Figure 7. Trapezoidal Wind Profile With Opposite Gradients of
Magnitude u_1' Spaced a/2 Along the Meteor Trajectory
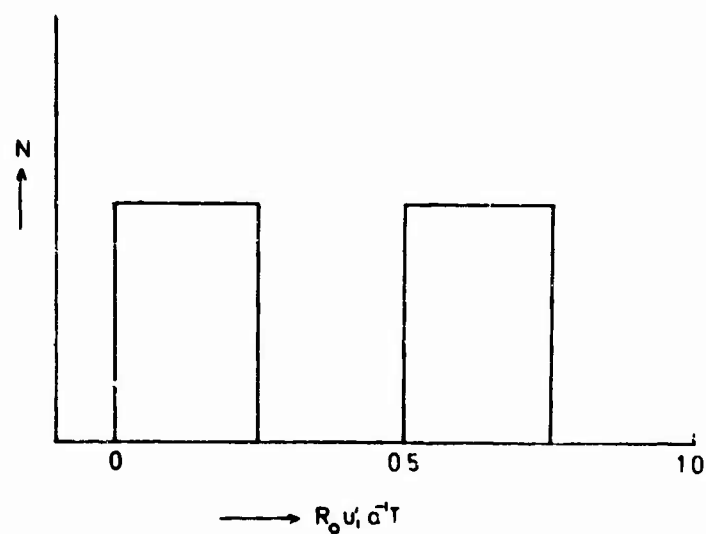
Figure 8. Distribution in Time Delay,
Trapezoidal Wind Profile

Thus, when expressed in terms of the wavelength a and the maximum gradient u'_1, the mean time delay is also relatively insensitive to the precise form of the wind profile.

2.3 The Depth of Fading

To evaluate the depth of fading we need to evaluate the amplitudes of the echo components at the reflection points $x = x_o$, $x = x_1$. Now, if the electron line density at a reflection point is α, the echo amplitude is that which we would obtain from q electrons scattering coherently and in phase, where $q = \alpha \ell$ and ℓ is the effective Fresnel zone length. Thus the echo amplitude is proportional to ℓ . Now, provided α does not vary too rapidly with x, we obtain

$$\ell = \left| \int_{-\infty}^{\infty} e^{-j\phi(x)} dx \right|, \tag{22}$$

where, from (3),

$$\phi(x) \simeq \frac{2\tau}{\lambda} \left(\frac{x^2}{R_o} + 2tu(x) \right) . \tag{23}$$

Let $\xi = x - x_n$ where $n = 0, 1$ and expand ϕ as a power series, taking $\phi = 0$ at $\xi = 0$, remembering that $\phi'(\xi) = 0$ is the condition for reflection. Thus

$$\phi(\xi) = \frac{\xi^2 \phi''(o)}{2!} + \frac{\xi^3 \phi'''(o)}{3!} + \ldots . \tag{24}$$

For the zero order reflection, $\phi''(o)$ will in general be non-zero, so, neglecting higher terms,

$$\ell_o = \left| \int_{-\infty}^{\infty} e^{-j\phi'' \xi^2/2} d\xi \right| = (2\pi/\phi'')^{\frac{1}{2}}$$

$$= \left[\frac{\lambda R_o}{2(1 + u''R_o T)} \right]^{\frac{1}{2}}, \tag{25}$$

where u'' is the value at $x = x_0$.

Now the first glint, when it appears, satisfies (7), and hence, from (23), $\phi''(x_1) = 0$ at $t = T$. This means that there is relatively strong focusing and we must include the higher terms in the expansion (24). Provided terms in ξ^4 and higher can be neglected (which generally will be so for a fairly smoothly varying wind profile) we get

$$\ell_1 = \left| \int_{-\infty}^{\infty} e^{-j\phi'''\xi^3/6} \, d\xi \right|$$

$$= \frac{(6/\phi''')^{\frac{1}{3}} \Gamma(1/3)}{\sqrt{3}}$$

$$= \frac{1}{\sqrt{3}} \left(\frac{6\lambda}{4\pi u''' T} \right)^{\frac{1}{3}} \Gamma(1/3) \tag{26}$$

where u''' is the value at $x = x_1$.

The expressions for the sinusoidal case are obtained by substituting $u'' R_0 T = -Z \cos y_0$ in (25) and $u''' T = kZ R_0^{-1} \sin y_1$ in (26). The ratio in echo amplitudes, ℓ_1/ℓ_0, thus depends on the wavelengths both of the radio waves and of the wind profile; it may be greater than unity in this case. When $b = 0$, we have $Z = 1$ and $y_0 = y_1 = 0$; thus (25) and (26) both become infinite. This is because ϕ'' is zero and higher terms must be included. It is not of great importance, however, since the distribution position for Z is zero at $Z = 1$. It is the limiting case, where the zero order reflection and the first glint coincide, and the initial fading frequency is zero; whereas, from Sections 2.1 and 2.3 we expect the observed distribution in fading periods to be closely grouped about the values given by Eqs. (18) and (19).

In the quasi-trapezoidal case, we would, from geometrical considerations, expect ℓ_1 to be of the same order as, or less than, ℓ_0 at the onset of fading.

The above reasoning can be extended to investigate the variation in amplitude with time of the zero and higher order reflections. It would be of interest to compute this for model wind profiles and to compare the results with the observed echo profiles.

2.4 Spaced Receiver Observations

Additional information about the wind profile should be available from simultaneous observations of enduring echoes with spaced receivers. For instance, one can transmit on an aerial with a relatively narrow beam and use three receivers spaced distances ℓ_L and ℓ_T along and transverse to the beam axis. Following the onset of fading the zero and first order reflections will produce an interference pattern on the ground leading to a displacement in time of the fading patterns. If this displacement is a fraction q_L and q_T of the fading period for the longitudinal and transverse pairs, then the distance between the reflection points along the train is

$$S = (S_L^2 + S_T^2)^{\frac{1}{2}},$$ (27)

where $S_L = q_L R_o \lambda / (\ell_L \sin \theta)$ (28)

and $S_T = q_T R_o \lambda / \ell_T$, (29)

and the height interval between the reflection points is

$$\delta h = q_L R_o \lambda \ell_L^{-1} \cot \theta .$$ (30)

The elevation, θ, to the reflection points is given, to sufficient accuracy, by

$$\sin \theta = \bar{h} / R_o$$ (31)

where $\bar{h} \sim 95$ km is the mean height of the enduring echoes.

The difference in the line of sight drift between the two reflection points is related to the fading frequency by

$$\delta u = \lambda \nu / 2$$ (32)

and, assuming a horizontal wind with component w in the direction of the aerial beam azimuth,

$$\delta w = \lambda \nu / (2 \cos \theta) .$$ (33)

Hence the mean shear in this component, $\delta w/\delta h$, can be obtained. The distribution in values of δh will yield information concerning the form of the wind profile and average magnitudes of δh and δw can be related to the vertical scale of the profile and its amplitude. From measurements of S_L and S_T we also obtain the orientation of the train in the plane normal to the ray path and hence, may deduce the meteor radiant to within an accuracy set by the azimuthal beamwidth.

A rough estimate suggests that, for λ 10 m, spacings ℓ_L of half a kilometer and ℓ_T of rather less would be adequate.

3. DISCUSSION

The possibility of obtaining useful wind data from fading observations is attractive because of their simplicity as compared with the Doppler technique, which requires a phase-coherent radar system and a relatively complex technique for display and analysis. It should indeed be possible to use a conventional ionosonde at the upper limit of its frequency range with a simple dipole-reflector or Yagi aerial at a suitable height to produce a beam at about 20° elevation.

Enduring echoes are easily recorded on magnetic tape and can be displayed for analysis on a large screen cathode-ray tube with a persistent phosphor. This method has been used successfully at Sheffield with a dual track tape, one track recording pulses synchronised with the transmitter pulses, and the other recording the echoes (with the transmitter pulses suppressed).

It is clear from Eqs. (18), (19), (20), and (21) that the parameters u_1 and a/u_1', deduced from measurements of $\overline{v}$ and $\overline{R_oT}$, are relatively insensitive to the precise form of the wind profile. Hence the method should prove of value in determining diurnal, seasonal, and geographical variations in these quantities. Preliminary analyses of echoes obtained during the IQSY at Halley Bay, Antarctica, and at Sheffield seem to reveal significant differences in the nature of the wind profiles. The data appear to give reasonable values in the wind parameters when interpreted according to the above theory; however, there are some unexplained features. The distribution of values of R_oT deviates significantly from both of the predicted ones, and the values of R_oT appear to increase with increasing R_o. It is not yet clear to what extent these deviations may be due to the deficiencies in the theory arising from the simple models considered, or to observational selection. The Doppler technique, using spaced stations, clearly is the more attractive for studying in detail the wind structure in the meteor region, particularly since the wind shear can be determined from individual echoes as discussed in Section 1. It is hoped to extend such observations to equatorial latitudes through a cooperative development between the Space Physics Group in Sheffield and the British

Meteorological Office. Nevertheless, the technique is a highly specialized one and is likely to remain for some time limited in its geographical coverage. It is therefore suggested that those with suitable facilities available (ionosonde stations for instance) be encouraged to initiate fading studies along the lines suggested. If sufficient support is forthcoming, an observing program should be drawn up which will ensure simultaneous observations at a number of widely spaced locations.

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IX. Upper Atmospheric Wind Observations at Adelaide

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Abstract

The main features of upper atmospheric winds determined from radio observations of drifting meteor trails carried out at Adelaide are described. The seasonal behaviour of the prevailing, and 24- and 12-hour periodic components over the height range 80 to 100 km has been established. A weak but significant 8-hour periodic component has been revealed by spectral analysis.

Small scale turbulence has been studied using several spaced stations; one notable result is the semi-annual variation in the rate of dissipation of turbulent energy.

1. INTRODUCTION

A study of the motion of the upper atmosphere by radio observations of drifting meteor trails has been carried out at Adelaide, South Australia, since 1952. The radio system used for this purpose has been described in a companion paper (Elford, 1966). The main emphasis of this study has been to determine the

systematic behaviour of the winds throughout the height range 75 to 105 km and the variation of these winds as a function of the time of the year (Elford, 1959).

In 1960 the system was extended by the addition of spaced receiving sites so that simultaneous measurements of drift could be made of several reflecting centers on the one meteor trail. From a comparison of the relative motion of these centers, measures of both the scale and energy of the turbulence at these heights could be made. A survey carried out during 1961 revealed a significant semi-annual variation in the rate of dissipation of turbulent energy at 90 km (Roper and Elford, 1963).

The 1961 wind survey also gave a much more detailed picture of the annual variation in the systematic winds than had been obtained before. In this paper some of the outstanding features of wind results obtained at Adelaide since 1960 are discussed.

2. METHOD OF ANALYSIS

The raw data available for analysis consists of measures of the line of sight drift of segments of meteor trails whose absolute position is known to an accuracy of about 2 km in height and 4° in azimuth. (In the case of two or more reflecting regions on one meteor trail, the relative distance between reflecting centers is known to an accuracy of 100 m).

The deduction of the systematic features of the actual wind from the observed line of sight velocities is carried out using a method of analysis developed by Groves (1959). In this method it is assumed that, in general, the EW, NS, and vertical components of the actual wind can each be expressed as certain specified functions of height and time. For data extending over several days, the wind is assumed to be periodic in time and to have a polynomial variation in height. As an example, the EW wind component is assumed to be of the form

$$u = u_0(z) + \sum_{j=1}^{p} \left\{ u_j(z)\sin jwt + u_j^*(z)\cos jwt \right\}$$

and

$$u_j(z) = \sum_{i=0}^{A_j} u_{ji} S^{A_j}, \qquad u_j^*(z) = \sum_{i=0}^{A_j} u_{ji}^* S^{A_j},$$

where

$$S = \frac{2h - (h_{max} + h_{min})}{h_{max} - h_{min}}$$

and h_{max}, h_{min} are the limits in height of the data. The NS and vertical components are assumed to have a similar form.

The choice of the period $2\pi/\omega$ depends on the wind components being sought. Usually the fundamental period is chosen as 24 hours and p is given the value 3. However, the analysis is completely general and any fundamental period can be chosen. Thus it is possible to carry out a spectral analysis of the wind data. Some examples of this type of analysis are discussed in Section 4.

The values of the coefficients u_{ji}, u^*_{ji} and so forth, which give a wind model that best fits the experimental data are determined by the method of least squares. In a typical analysis of data say extending over ten days the number of coefficients may range from 70 to 100. The standard deviation of each coefficient is also determined and a comparison of the magnitude of a coefficient with its standard deviation is used to estimate the significance of that particular coefficient.

For observations extending over intervals of time of the order of one hour the least squares analysis is carried out assuming no variation in time.

As a result of the diurnal variation of the meteor echo rate the data used for wind determination is unevenly distributed throughout the day. The situation is worst between July and September when the ratio of maximum to minimum rate is approximately 7:1. During these months it is difficult to obtain a reliable estimate of the diurnal features of the wind from less than ten days continuous observations.

3. SYSTEMATIC WINDS

The chief characteristics of the systematic winds determined from the observations at Adelaide can best be seen from an examination of the mean and periodic wind components determined during a survey carried out from December 1960 to August 1962. During each month meteor drifts were recorded for periods ranging from 7 to 19 days and analysed according to the methods described in Section 2.

3.1 Mean Zonal

The mean zonal winds determined during the survey are shown for three height levels in Figure 1. The bars about each plotted point represent the rms deviation. It can be seen that over the height range 80 to 100 km the zonal wind is predominantly toward the East. The only strong wind reversal occurs at the upper level where the wind is toward the west during the winter of 1961. No significant reversals occur at the other levels but the wind has its minimum eastward amplitude in the spring at 91 km and in the summer at 83 km. As a result of the rapid change in the zonal wind with height the seasonal patterns at 83 and 99 km are almost

ZONAL WINDS
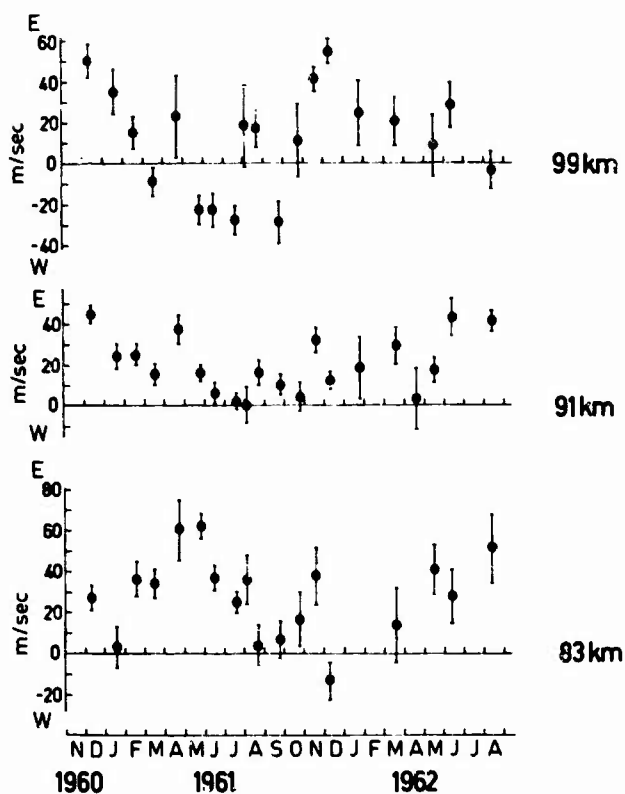
Figure 1. Seasonal Variations in the Mean Zonal
Winds at Three Heights

opposite in phase. This behaviour is also reflected in the wind gradients, which
have maximum values of +4m/sec/km in summer and -4m/sec/km in winter.

There is some evidence of an annual repetition of the mean zonal wind at the
99 km level but at 83 and 91 km the half-period for long term changes is probably
10-11 months.

3.2 Mean Meridional

In contrast to the zonal winds, the meridional winds shown in Fig. 2 exhibit
an annual behaviour which is similar at all levels. In general, northward winds
occur during summer and southward winds during winter. A similar meridional
variation is found for these levels at Jodrell Bank 53°N and at Mawson 68°S. Thus
the meridional flow at these levels is consistent with a horizontal movement of air

MERIDIONAL WINDS
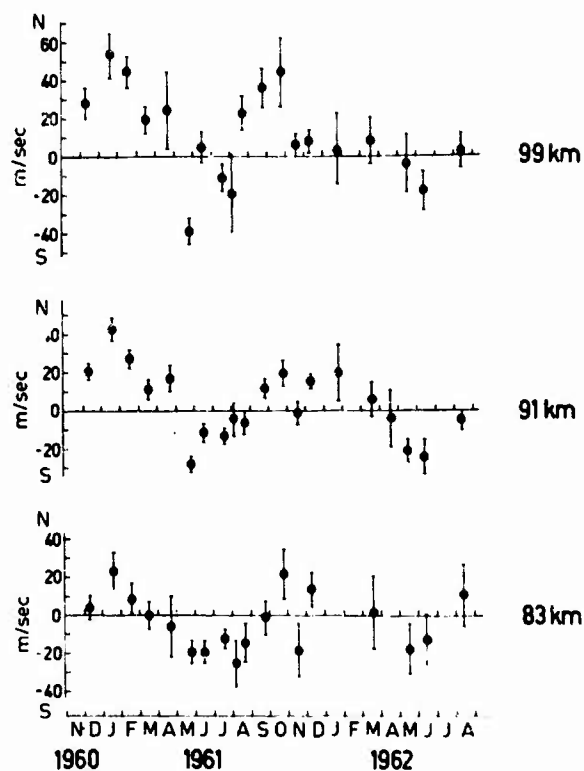
Figure 2. Seasonal Variations in the Mean
Meridional Winds at Three Heights

from the summer to the winter pole. The Adelaide results show that the mean
meridional wind increases with height over the range 80 to 100 km and that above
90 km the amplitude of the mean meridional wind is comparable with that of the
mean zonal wind.

3.3 Twenty-four Hour Periodic

A detailed investigation of the phases of the EW and NS diurnal components for
each month indicates that the wind vectors rotate anticlockwise as is required for
tidal motions in the southern hemisphere. The main features of the annual be-
haviour of these components are best illustrated by grouping the months into sea-
sons and determining the mean value for each season. The results for 1961 are
shown in Fig. 3. The radius of the error circle is a measure of the RMS deviation

200

DIURNAL WINDS '961 (LAT. 35°S)
HARMONIC DIALS

99 km

91 km

83 km

EAST-WEST.   NORTH-SOUTH
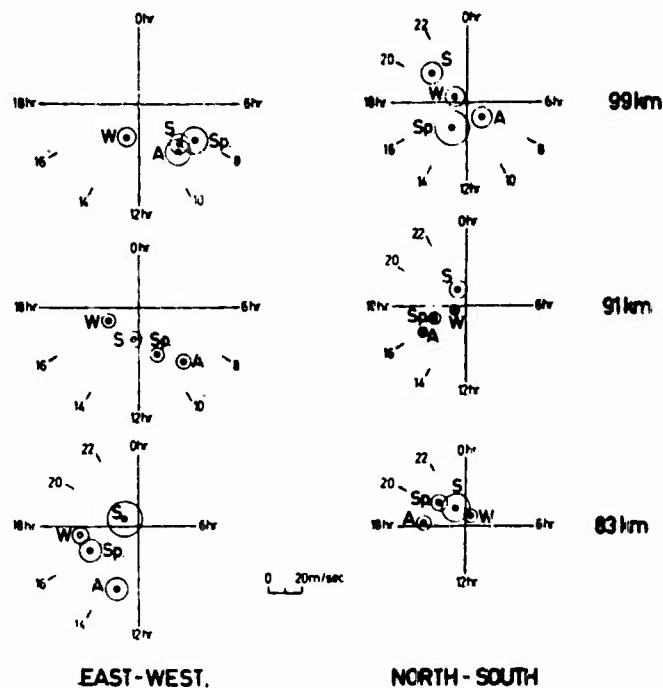
Figure 3. Seasonal Variations in the Diurnal
Periodic Components at Three Heights

in each component. It can be seen that the phase of the 24-hour components ad-
vances with height particularly at the equinoxes when the phase advances by 6 to
9 hours over a height change of 16 km. This is equivalent to an advance of about
7° per km.

3.4 Twelve-Hour Periodic

The mean seasonal values of the semi-diurnal components for 1961 are shown
in Fig. 4. The most marked feature of these results is the reversal in phase from
summer to winter with maximum amplitudes occurring at these seasons and mini-
mum amplitudes at the equinoxes. The magnitude of the semi-diurnal component
in winter and spring shows a strong positive height gradient, but the phase shift
with height is relatively small at all seasons.

HARMONIC DIALS

99 km

91 km

83 km

EAST-WEST   NORTH-SOUTH
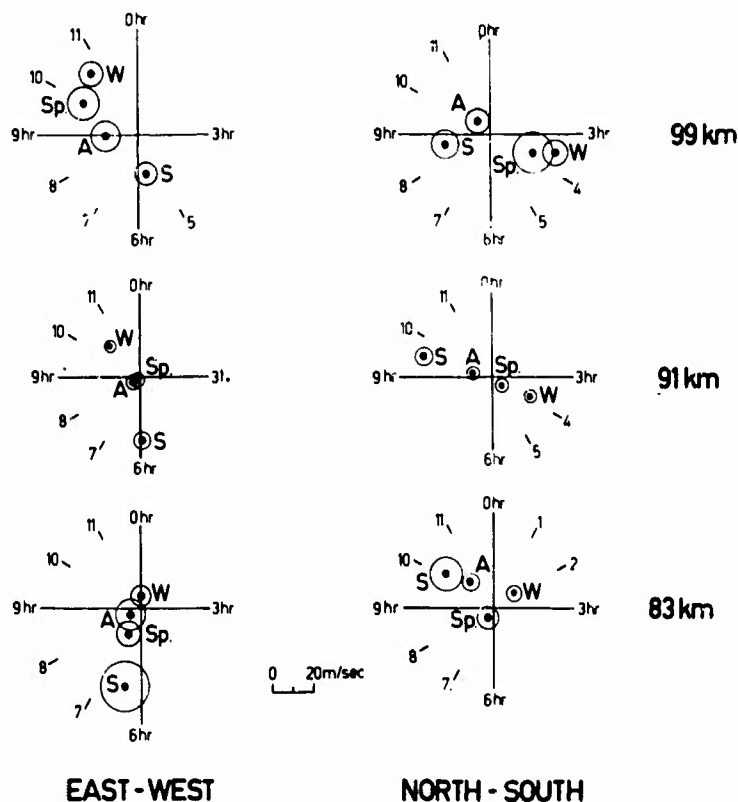
Figure 4. Seasonal Variations in the Semi-Diurnal
Periodic Components at Three Heights

4. PERIODOGRAM ANALYSIS

The magnitudes of the 24- and 12-hour periodic components have always been
sufficiently large that there has never been any question of their validity in the
Adelaide results. The possibility of there being a significant 8-hour wind compon-
ent had never been discounted but the least squares analysis showed that in general
its magnitude was only marginally greater than the rms deviation. In order to test
the reality of this component and to look for any other significant periodic compon-
ents each month of the 1961 set of observations was subjected to a frequency analy-
sis. This was done by determining the amplitudes and phases of all periodic com-
ponents in the range 0.5 cycles/day to 4 cycles/day in increments of 0.05 cycles/
day.

To assist in identifying significant peaks in the spectrum the wind energy per
unit mass was calculated for each frequency by forming the sum of the squares of

the amplitudes of the zonal, meridional, and vertical components of that particular frequency. A typical result is shown in Figure 5 for July 1961 for the heights 83, 91, and 99 km. The spectra are dominated by a group of peaks near 24 hours and a single strong peak at 12 hours. The complex nature of the periodogram around 24 hours indicates that the diurnal component varied markedly in amplitude and phase from day to day during the period of observation, a result that has been confirmed by an investigation of the wind pattern on a number of individual days.

To investigate the significance of the 8-hour wind component throughout a complete year, the energy spectra for the twelve months have been averaged to give the result shown in Figure 6. Again, the 24- and 12-hour components are dominant and the 8-hour peak remains. It is thus considered that the terdiurnal oscillation is a real feature of the winds in the 80 to 100 km region. A careful inspection of all the spectra did not reveal any other significant peaks.

A number of other features are shown by the spectra in Figure 6 and are worthy of note. On the average the diurnal wind component remains constant in amplitude at all levels while the semidiurnal component increases in amplitude with height. As mentioned above, this increase in the strength of the 12-hour component with height mainly occurs during spring and winter.

The width of the spectral peaks depends on the length of the records being analysed. For an average duration of 10 days the natural base length of a spectral peak is 0.2 cycles/day. The greater degree of irregularity in the background of the spectra at the 83 and 99 km levels as compared with the 91 km level is a consequence of the fall off in the density of the data above and below 91 km.

5. TURBULENCE

From observations of the relative line of sight motion of two or more reflecting regions on the one meteor trail it is possible to determine the mean square transverse velocity difference between points separated by a distance r. Turbulence theory shows that for an isotropic and inertial region this quantity, usually termed the structure function, should vary as $r^{2/3}$ and the constant of proportionality gives a measure of the rate of dissipation of turbulent energy in the region.

Application of the structure function method of analysis to multistation meteor drift observations leads to two different results depending on whether the separation is taken in the vertical or in the horizontal directions. Typical results for May 1961 are shown in Figures 7 and 8. In the case of vertical separations the exponent of r is about 1.4 while for essentially horizontal separations the exponent is approximately 0.7, a value that is quite close to that predicted by theory. The more rapid variation of the structure function with vertical separation has also
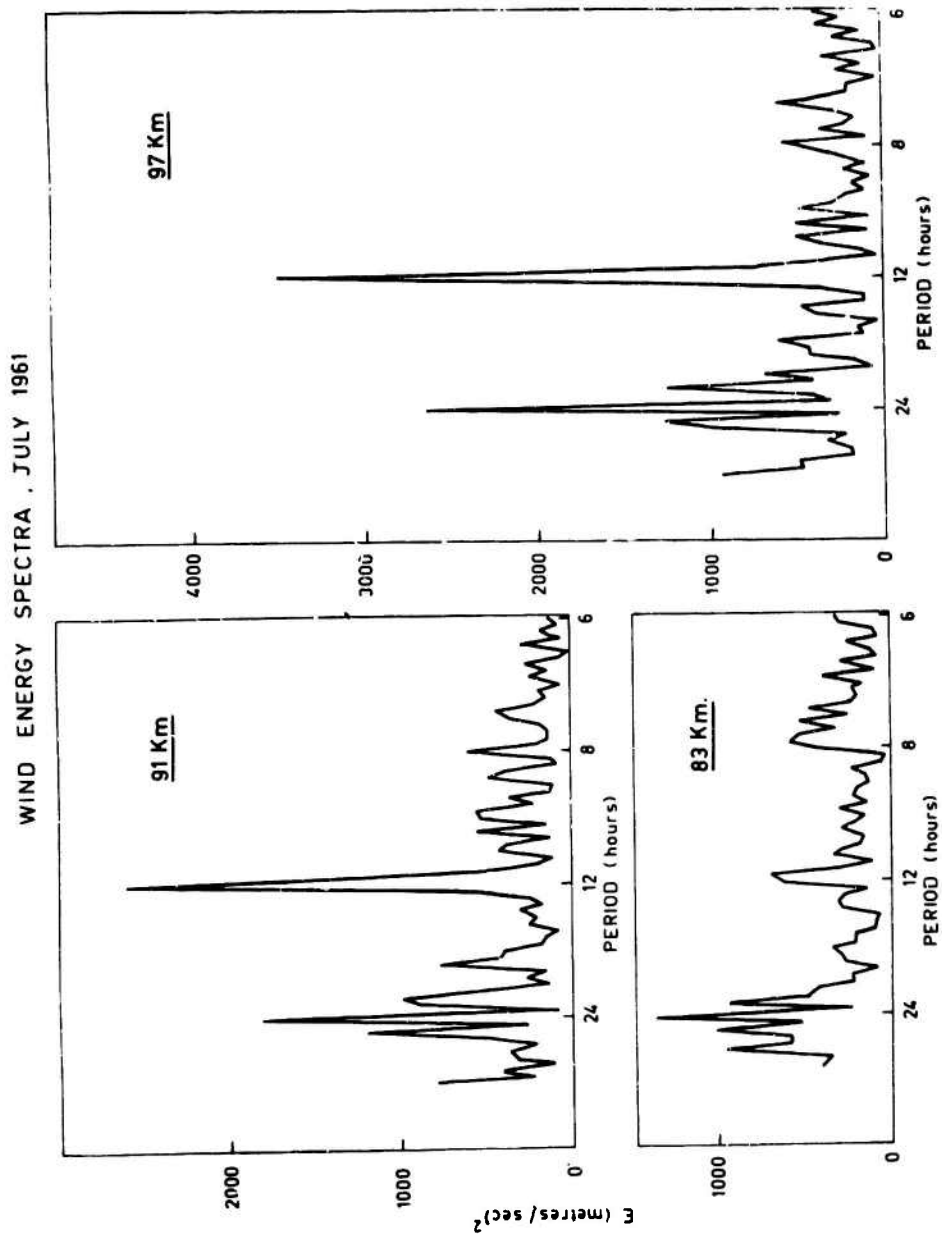
Figure 5. Spectra of the Wind Energy per Unit Mass at Three Heights for the Period
July 12 to 31, 1961
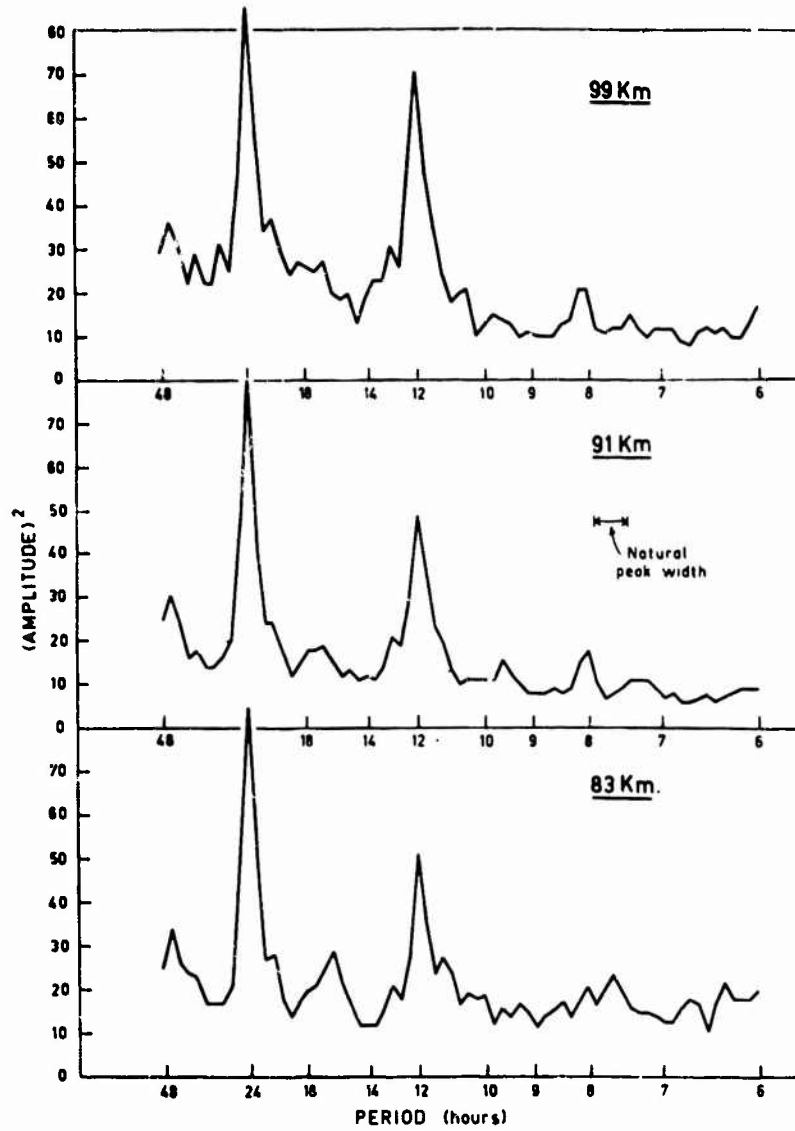
Figure 6. Average Spectra of the Wind Energy per Unit Mass at Three Heights for the Period January to December 1961

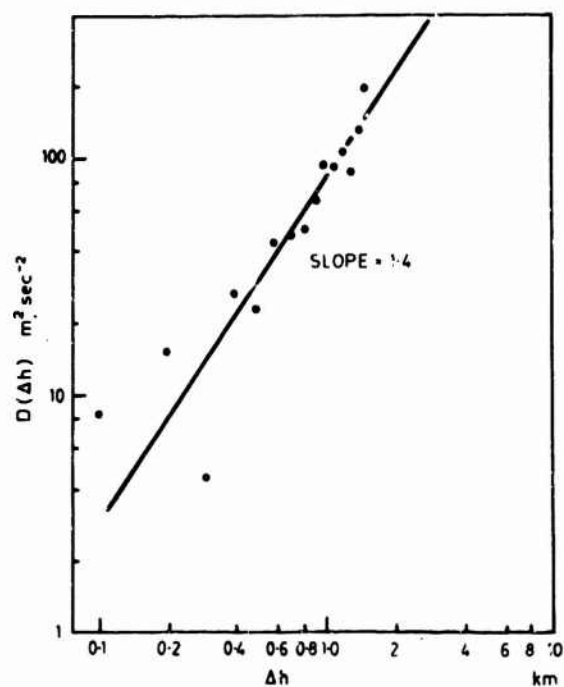Figure 7. Variation With Height Difference Δh of the Structure Function D(Δh) Determined From Radio Meteor Trail Shears at Adelaide (35°S) During May 1961
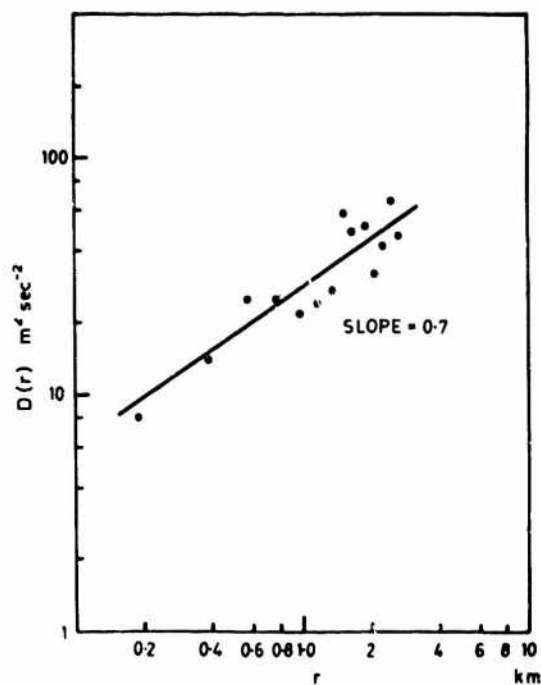
Figure 8. Variation of the Spatial Structure Function Determined from Radio Meteor Results for May 1961

been observed in the case of chemiluminescent trails laid by rockets. In view of the inhibition of vertical motion by buoyancy forces it is probable that the height dependence of the structure function is related to the gravity wave spectrum at these levels rather than to turbulence. A more detailed examination of the meteor drift observations shows that in the height range accessible to measurement, 80 to 100 km, the turbulence is markedly anisotropic for scales larger than a few kilometers.

Values of the rate of dissipation of turbulent energy have been estimated for each month of 1961 using the spatial structure function. The results are shown in Figure 9, together with measures of the kinetic energy associated with the components of the systematic wind. There is a pronounced seasonal variation in the rate of dissipation of turbulent energy, minimum values of 1.5×10^{-2} watts/kgm occurring in the summer and winter with maxima of 3.5×10^{-2} watts/kgm at the equinoxes. It is interesting to note that this variation appears to be strongly correlated with the seasonal variation of the 24-hour component of the mean wind. A further survey will be necessary in order to establish the reality or otherwise of this correlation.

6. DAY TO DAY VARIATIONS

Until recently the meteor echo rate obtained with the Adelaide system was, in general, insufficient to enable the main features of the wind to be determined from a single day's observations. In principle a continuous behaviour of the wind during a given observing interval can be obtained by a Fourier transform of the frequency spectrum of the amplitudes of the zonal, meridional, and vertical components. The extent to which this transformation gives a meaningful result depends on the meteor echo rate in relation to the highest frequency in the spectrum. Thus the wind patterns will be least reliable at times of low echo rate. Several of the periods of observation during 1961 had sufficient data to enable this transformation to be applied. Since the longest period in the spectrum was 48 hours the result of the transform was essentially a record of the day to day variations in those components with periods shorter than about two days. Figure 10 shows the zonal and meridional winds obtained in the manner described above for three successive days during September 1961. The striking feature of this result was the variability of the wind pattern from day to day at all levels.

The extent to which the day to day variability is due to turbulence, gravity waves, or large scale "weather systems" is not known. It is hoped that current observations will allow wind profiles to be determined hour by hour and thus lead to a more detailed study of upper atmosphere wind variability.

In order to obtain the amplitude of the signal received from a section of the trail we integrate and get:

$$A_R = (2r \, \Delta P_R)^{1/2} \quad q \int_{s_1}^{s_2} \sin (2\pi ft - \frac{4\pi R}{\lambda}) \, ds,$$

It is at this point that we make the assumption that q is constant along the trail. The question is, how constant is q and can we, in fact, move it outside the integral. Peter Forsyth did some work on this. What did you conclude, Peter?

DR. FORSYTH. The evidence points to considerable variation of q along the trail. The work we did consisted of reflecting four frequencies off of a trail and obtaining four different decay rates. Most of the energy is reflected from the first Fresnel zone and since the four frequencies were not all close together we obtained the integrated contribution from four different lengths of trail centered about the specular reflection point. The four decay rates (for underdense trails) were generally not the same, indicating that there were inhomogeneities in the distribution of q along the trail.

Additional confirmation of this came from studies of the actual formation of the trail by looking at the Fresnel patterns as they were being formed even before the specular reflection point was reached. This allows you to get the amplitude from small sections of the trail. This has fluctuations in it, fluctuations on the order of ten, perhaps more. That is quite enough to weight that interval significantly for different frequencies; it means in fact that while you still have the Fresnel zone on the trail, the actual scattering is weighted heavily. There is a kind of resonance between your scattering wave and the inhomogeneity in the trail, the irregularity. This, I think, is an important factor. This is why you can't get reasonable decay rates.

DR. PETERSON. When you were talking of the Fresnel pattern, were these large meteors?

DR. FORSYTH. These are all underdense. They are all at least as linear a decay as you could find.

DR. SOUTHWORTH. What is the length of scale of these irregularities?

DR. FORSYTH. Well, the ones that are important, that showed in the study, say, five or six irregularities per Fresnel zone.

DR. SOUTHWORTH. What wavelength?

DR. FORSYTH. The Fresnel zone that we are talking about is one or two kilometers up to five or six kilometers. So, the irregularities are on the order of hundreds of meters.

DR. SOUTHWORTH. I th"'k that it is something of this sort. The point is that we missed this irregularity in ionization, and I realize now that it is quite within the possibility of a physical displacement.

THE FLOOR. Displacement you need.

DR. FORSYTH. That is right.

THE FLOOR. You need this on the interval, and it is only a matter of a meter or a hundred meters.

DR. BARNES. Displacement due to winds?

DR. FORSYTH. If you have turbulence, it is on the size of the order of a hundred meters.

DR. SOUTHWORTH. If all you need is a meter displacement, then the theory we are using here for the formation of this trail is not good due to the lateral shift. On the other hand, the irregularities may be primarily due to the fragmentation. This is one of the real problems that we hope to attack with the Havana system. That is one of the things we are really looking for, fragmentation, and there can be a lot of irregularity on this scale.

DR. BARNES. Now, to summarize, what you are saying then is that you observed variations in the Fresnel pattern?

DR. FORSYTH. In the amplitude, essentially due to the derivative of the incremental part of the Fresnel.

DR. BARNES. The point I want to make is, if you do say it is caused by displacement, has this really changed the electron concentration? That is the point we are trying to make?

THE FLOOR. Well, you don't know what part of the integral is affected, whether it is q or the retardation.

DR. ROPER. The change is in the distribution of q.

DR. PETERSON. Many years ago when we did these Fresnel spirals on the oscilloscope, and the French system still displays the Fresnel pattern, even the formation one, I guess, ---.

DR. REVAH. Not yet.

DR. PETERSON. Well, when w· were doing that, instead of being a smooth spiral, they had these little fluctuations away from it. There were times when it was purely radial, and there were other times when they were phase. Some of those were pretty good.

DR. SOUTHWORTH. You mean pretty clean?

DR. PETERSON. Pretty clean.

DR. FORSYTH. Again, the effect doesn't have to be very large. All we are doing is looking at a relatively small vector and adding to a larger vector, and that small vector itself is fluctuating by the order of 10 percent as it rotates with the phase. This is the thing that we look for.

DR. ELFORD. What sort of lateral velocity do you require?

THE FLOOR. Not much. Certainly 50 meters per second.

DR. ELFORD. This is 50 meters per second relative to another portion, say a few hundred meters away, so you get a strong shear?

THE FLOOR. We are talking about the scale size on the order of a few hundred meters, one hundred to two hundred meters in length. It is of this order, and displacement is of the order of a meter or two, which would be what you need. All you need to do is to look at the phase term in this integral.

DR. BARNES. Do you think that there is a possible way of sorting out those which would give you good decay data?

THE FLOOR. I think there is. Some are pretty clean.

DR. PETERSON. Even if it fluctuates a little bit during the formation process, how sure are we about the integrated effect when the trail is formed?

THE FLOOR. This you can check out very quickly. Rice just did a study working for me along a line of sinusoidal fluctuation and if you add these things together you can easily generate trails for which the actual centers of gravity, if you like, of the return signals are shifted by kilometers for two different frequencies and separated by 20 percent.

DR. SOUTHWORTH. You are thinking of forward-scattering?

THE FLOOR. I am. But, the problem is essentially the same for backscattering.

DR. SOUTHWORTH. Well, we are taking the Fresnel oscillation from the Fresnel pattern and deriving the ionization point by point. So far the ones I have been producing, I don't have faith in.

THE FLOOR. What you need is a whopping big signal-to-noise ratio and a fine scale in the sampling. The sampling must be done, obviously, many times per Fresnel zone.

DR. SOUTHWORTH. This I require routinely anyway. Well, that is, I get down to about five samples per Fresnel zone.

THE FLOOR. You see, five samples per Fresnel zone would just barely get you redundancy. It would give you the resolution when you are luckiest, or rather most fortunate, just to resolve the kind of fluctuation discussed here.

DR. SOUTHWORTH. The fluctuations I see are perfectly clear and easy to measure.

THE FLOOR. No, the fluctuations within the Fresnel zone are the parts that we are looking at.

DR. SOUTHWORTH. I am going to look at the amplitude of the little oscillations. Certainly most of our curves do not follow the pattern computed for uniform ionization.

DR. PETERSON. Let's see, somebody needs to pinpoint why we are worried about these fluctuations.

DR. FORSYTH. Well, if you are going into decay rates, this affects the decay very much.

DR. PETERSON. Well, the displacement doesn't affect it very much, but the variation in q does.

DR. FORSYTH. In either case, it weights the point on the trail from which you are getting the signals. Let's put it this way, if you take measurements with three or four frequencies from the same trail, you get decays which are essentially linear in the logarithmic recording, but they differ in the diffusion coefficient. They differ by the same order as the scattering in diffusion measurements for trails at the same height.

DR. PETERSON. I think I believe that, but the wind itself is going to displace the whole thing, so it goes through a number of variations.

DR. FORSYTH. If we are talking about this effect arising through sinusoidal or relative displacement on the trail, then it depends upon how high you are making your measurement. Then you may have a decay rate that is weighted one way for one trail and quite differently for another trail. It is conceivable, for example, that both of the signals come from the top and bottom edge of any trail, rather than from the center of any Fresnel zone.

DR. PETERSON. I was asking whether the wind shears didn't make up a bigger factor of distortion than the formation portion of it.

DR. ROPER. I think the wind shears can. This is something that you can visualize. In some cases, for instance, the wind shears do pull the trail apart. If I interpret what I saw correctly, you are talking about the small scale turbulence?

DR. FORSYTH. Well, I don't know that I am talking about this.

DR. SOUTHWORTH. One thing that I find is important with these long Fresnel zones is that you are going to have a range of diffusion rate within it, depending upon the height center of gravity, so that essentially the longer the wavelength you use, the lower the diffusion temperature.

DR. FORSYTH. It isn't big enough to account for it.

DR. ELFORD. The displacement is surely related to velocity as the trail is drowned out. I can't visualize the relative numbers here. Has anybody done this?

DR. PETERSON. I thought the time after the wind, or the overblowing of the wind, over-weighted the other factor.

THE FLOOR. I don't understand how you can get a linear decay out of a trail distorted this strongly by so much. Dr. Ellyett, if I may comment here, it is a considerable number of years since we set out in New Zealand to measure velocity of individual meteors from the Fresnel diffraction pattern, and so few, on the order of between one in 20 and one in a hundred, were clean enough to be handled that

we gave this up. We found that the individual Fresnel zones patterns varied in their individual sizes; in other words, they weren't fitting the theory correctly. This theory was explained very clearly by Kaiser in one of his early papers. The zones were the wrong relative length and we found we couldn't use alterations like that in a systematic way to get the velocity. It was on the order of about one in 20 to one in a hundred that gave a good clean sort of curve.

DR. SOUTHWORTH. These are large meteors?

DR. ELLYETT. These are underdense at a frequency of 69 MHz.

DR. SOUTHWORTH. Most of our underdense meteors we could measure very well in the Fresnel pattern.

DR. PETERSON. How much do they fluctuate in scale of half periods, or whatever fraction of a scale you used?

DR. SOUTHWORTH. The fluctuation frequency was approximately five percent of the total amplitude.

DR. PETERSON. No, not the amplitude, the period. How accurately do they fit according to the Cornu spiral?

DR. SOUTHWORTH. Our spiralling would go on spiralling. I am not expecting that it will have any coils uniformly spaced with one within the other.

DR. BARNES. You can get the velocity by smoothing over a whole lot of mean reflection even if these fluctuated in between?

DR. SOUTHWORTH. Yes.

DR. ELFORD. In Adelaide, we observed the Fresnel pattern before the T_0 point by beating in a continuous wave system, and there we find that the waves do follow a theoretical relationship to the maxima and minima functions of time. This is perhaps better than the T_0 positive signal.

DR. SOUTHWORTH. We are looking at both sides of T_0.

DR. MILLMAN. In McKinley's velocity work, I don't think that any particular attention was paid to the irregularity in the Fresnel pattern, but looking at most of his, and I have looked at thousands of his Doppler patterns, they look very regular. However, it must be remembered that those meteors are on the borderline between the overdense and the underdense trails. They go down to about eight magnitude, visual, and, where he measured some thousands of meteor velocities, the trails always looked very regular. It is certainly the record you get from the head of the meteor coming down which is a regular thing, as is shown by the head echoes. And right at the start, you do have a regular record to measure. Now, we have done nothing very much in the line of studying what happens in a fraction of a second after that, because our technique doesn't cover that type of study.

DR. ELLYETT. I was just going to say that I think you have to be fairly careful when you actually take your measurements of them.

DR. SOUTHWORTH. I seem to have five or six points per Fresnel zone even down where they are uniformly decreasing.

DR. BARNES. Actually, the way to look at it would be with the cw in order to look at the variations. What is the PRF at Havana?

DR. SOUTHWORTH. 750.

DR. PETERSON. I was going to say that when we were displaying the vector quantity, I recall seeing fluctuations in the amplitude part of it (it is easier to see when you have a circle going around) and I would guess that I couldn't disagree with Peter that 10 percent wasn't a reasonable number.

DR. FORSYTH. Well, of course, that is a small fluctuation that was necessary in order to account for a very large variation of, say, decay rates.

DR. BARNES. Are there any more comments? Is that all? Then we should put q back underneath the integral.

MR. MÜLLER. I would like to make a fine point here that we might have to consider the effects of a plasma resonance. Not only is the phase change about 180 degrees, but also the amplitude changes. Theoretically, you get an excess of factor two. This, of course, will effect the decay rate, particularly if you operate at four different frequencies.

DR. BARNES. But how frequently do you get plasma resonance?

MR. MÜLLER. This is what I am trying to find out. It was shown by Kaiser that this may effect the decay.

DR. PETERSON. This depends upon polarization relative to the trail, which is tricky.

MR. MÜLLER. You get a fifty-fifty chance.

DR. BARNES. It doesn't last throughout the whole trail, but it would be nice if, when you got it, it would stay there while you get the decay.

MR. NOWAK. To return to the previous discussion, as I understood the argument it hinged mainly on the fact that by having irregularity in q over a Fresnel zone, you could weight the height towards the upper or lower end of the Fresnel zone. Now, if you had a fairly high frequency so that the Fresnel zone was fairly small, wouldn't that mean that even if you had an irregularity, it would be over such a fairly small height region that, since the decay is of an underdense trail, independent of q, it would still give you a height within your area of measurement. For example, if you could measure only the height to within ± 1 km, and you had a Fresnel zone of 1 km, even if you had irregularities within the Fresnel zone which would tend to shift your density height, your error in density height would be still masked partly by your error in the height determination.

DR. BARNES. Well, it turns out if you follow the equation all the way through, you make an assumption concerning the ambipolar diffusion coefficient and if you got blobs, you can't use this data, because you are diffusing in all three directions then.

DR. PETERSON. That depends upon how big your density variation is.

DR. BARNES. I don't have the numbers with me, but you can see the classical derivation would break down at this point.

DR. PETERSON. Yes, it would.

DR. BARNES. Well, it looks as though we should put q back under the integral. We are integrating over the trail and apparently we have run into one big stumbling block right here. Shall we go on and see where else we are going to run into trouble?

Now, in order to handle the integral, we have to make the assumption that range $R = R_o + s^2/2R_o$. Now, this looks like a very nice assumption, because the correction term is very small. The problem arises when you put it underneath the integral, because, when you start integrating the small term, it might be that the other terms cancel out and the integral of the small term that you have left is large. I haven't checked this to see if this is the case, but this is the reason that the meteorologists were stumped for so long. From the time that Richardson proposed his numerical equations until they were actually able to be implemented on a computer, this very fact, that some of the small terms were dropped too soon in the derivation of the equation, delayed for twenty years progress in this field. Hence it might well happen that the small term that is dropped here may be extremely important. I first realized this last week and haven't looked into the problem any further.

The next point in the derivation, following McKinley, is to make the substitutions

$$x = 2\pi ft - 4\pi R_o/\lambda \quad \text{and} \quad 2s = x(R_o\lambda)^{\frac{1}{2}} \text{ to obtain}$$

$$A_R = \frac{(2r\Delta P_R R_o \lambda)^{\frac{1}{2}}}{2} \, q \int_{x_1}^{x} \sin\left(x - \frac{\pi x^2}{2}\right)dx \ .$$

If we let

$$C = \int_{x_1}^{x} \cos\frac{\pi x^2}{2}dx \quad \text{and} \quad S = \int_{x_1}^{x} \sin\frac{\pi x^2}{2} \, dx$$

then we get

$$A_R = \frac{(2r\Delta P_R R_o \lambda)^{\frac{1}{2}}}{2} \; q(C \sin \, \chi - S \cos \chi) \, .$$

which takes us back to the Cornu spiral.

Are there any questions at this point on the derivation? I think it is straight-forward mathematics after the few things that I showed about R. This point will have to be investigated further.

DR. PETERSON. I think you are right, because it is a geometrical factor and not a tricky integral.

DR. BARNES. It turns out that you can't handle the integral unless you make this assumption, and it is just something that will have to be investigated. This is a purely mathematical problem. It is a small term, but there is the possibility that a small term discarded at the beginning may turn out to be very important later on after going through integrations and differentiations.

DR. SOUTHWORTH. The physical argument here is that the correction term corresponds to the moving trail by millimeters.

DR. BARNES. It is very small.

MR. NOWAK. It is just the optical case.

DR. PETERSON. No. No. In fact, if you want to do this whole job the hard way, you just take a very tiny volume and sum the whole business numerically. There are no big terms that are cancelling.

DR. BARNES. This is what the meteorologists thought for years, too.

For the power returned to the set we now get

$$P_R = \frac{A_R{}^2}{2r} = \frac{\Delta P_R R_o \lambda}{2} \left[\frac{C^2 + S^2}{2} \right] q^2$$

or, on substituting for ΔP_R

$$P_R = \frac{P_T G^2 \lambda^3 \sigma_e}{128 \pi^3 R_o{}^3} \left[\frac{C^2 + S^2}{2} \right] q^2 \, .$$

If the trail is long, then we can assume that we are working from the center point of the Cornu spiral, and, after the trail has been laid down, the term inside the square brackets will approach one. Now, the typical length of the trail is 15 km - - - -.

DR. SOUTHWORTH. How do you define length of your trail?

DR. BARNES. This is a good question, because q varies quite a bit from the beginning of the trail to the end of the trail.

DR. SOUTHWORTH. I think I am seeing (on the Havana system) for one magnitude below maximum magnitude, something like 6 km. If you go down two astronomical magnitudes, it is longer by 3 km.

DR. BARNES. You are talking about the magnitude of q ?

DR. SOUTHWORTH. Yes. Well, radio magnitude, astronomical magnitude.

DR. PETERSON. If you start with one that is 30 dB above the noise level, you then see a much longer trail.

DR. SOUTHWORTH. Yes, so it depends upon how you define your trail.

DR. MILLMAN. But, this is a very important point, because most of your results are going to depend upon how much or how many you pick up near the limit of your system. This is, I think, a very significant point. Your significant length is the length of the ones near the limit of your system.

DR. PETERSON. Well, not completely, because one can set the threshold quite a bit higher than the background noise in order to get a measure of accuracy.

DR. SOUTHWORTH. Well, this is not the limit of what I am getting the echo from.

MR. MYERS. What he is really saying is the length of the meteor trail is that set by the way you have your system set up, and you can vary this, of course.

DR. PETERSON. That is right. In fact, I guess you define the length by saying how much you are going to let the amplitude of the return signal vary.

DR. BARNES. Let's assume that it is 6 km long, then you are running into trouble, especially at the longer wavelengths.

DR. PETERSON. You are and you aren't, because you have only lengthened the amplitude by two to one.

DR. BARNES. But the thing is that you are not completing enough Fresnel zones, then.

DR. PETERSON. It is just how you define the length.

THE FLOOR. You said you were only interested in the factor of two to one.

DR. BARNES. Well, it looks like we have got trouble throughout this section.

DR. PETERSON. That q is certainly what is varying that, but it is varying smoothly. It is not chopping it off at the end of the zone.

DR. BARNES. All right, if you do get a couple of Fresnel zones, then $\left[\dfrac{C^2 + S^2}{2}\right]$ does approach one. We are going to assume that the q is constant.

DR. CHAMPION. You just defined the length of the trail in terms of the Fresnel zone. Isn't that right?

DR. SOUTHWORTH. No, No, I have 10 Fresnel zones.

DR. PETERSON. He says his length is 10 Fresnel zones in six km.

DR. SOUTHWORTH. Usually I have enough signal-to-noise, so I can go beyond this more characteristic length, but I almost never can tell when I see somebody else's trail as he defines it.

DR. PETERSON. Well, I think you are right.

MR. MYERS. What you are doing is setting a selection rule for your processing.

DR. BARNES. This is one of the rules that I have been trying to formulate. I was taking only those trails where I could see the Fresnel patterns. Are there any more questions before we go to the next slide?

DR. REVAH. We see the Fresnel zone pattern at the beginning of the echo and then we only see the decay at the end of the echo exponentially, but we don't see any continuation, maybe due to the last Fresnel zone on the trail.

DR. SOUTHWORTH. We sometimes have that, but more often there are some sort of fluctuations that go down until we pretty well lose them in the second Fresnel zone.

DR. PETERSON. Could it be that your frequency is getting high enough so that your bandwidth is cutting it out?

DR. REVAH. Yes, I suppose we are cutting frequencies off.

DR. PETERSON. Are those a hundred cycles per second or so?

DR. REVAH. We have only one hundred.

DR. PETERSON. You have a hundred cycle bandwidth.

DR. ELFORD. You can get up to 500 cycles.

DR. PETERSON. That is not unusual at all for Fresnel zone, but you wouldn't have it in your bandwidth?

DR. REVAH. Yes.

DR. PETERSON. Nor would we.

DR. BARNES. Now, are there any more comments?

The equation

$$\frac{\partial N}{\partial t} = \frac{D}{r}\frac{\partial}{\partial r}\left(r\frac{\partial N}{\partial r}\right)$$

is the standard form of the radial diffusion equation where D is the ion diffusion coefficient in square meters per second, N is the volume density of the electrons and r is the distance from the axis.

Now, right here we run into some trouble, because if we go high enough in the atmosphere, and if we are assuming that this is the ambipolar diffusion, the

atmosphere becomes thin enough so that the geomagnetic field takes over and you get the electrons diffusing in one direction faster than in another direction, and the same goes for the ions. Where this point is, is still a question.

DR. CHAMPION. Actually, you make the equation more applicable in the general form where the D goes inside the bracket. That formula is always correct when D is independent of r, and then D can vary depending upon your orientations.

DR. PETERSON. How far can your electrons get away from the ions?

DR. CHAMPION. This has nothing to do with it. I am just saying that this is not physics. This is mathematics. In the accurate mathematical formula, D is inside the brackets.

DR. BARNES. The question now is, in the meteor trail region is it important to take D inside?

DR. CHAMPION. That is a good question. I don't know. At 110 km it might be important.

DR. REVAH. I think it is important higher than 100 km.

DR. BARNES. This is the general feeling, that the breaking point is somewhere on the order of 100 km or a little above this.

DR. CHAMPION. There are also some practical demonstrations. For example, in the Sahara, they put up ions that are visible, and you can see ionization at certain altitudes. It is marked at 200 km. There is a question of how low you can come. The point is that across the magnetic field, when your diffusion coefficient is different by something on the order of one magnitude, so that even when your effect is small, you still have a major effect on what you are trying to achieve.

DR. ELFORD. This problem is important above about 110 km. Nobody worries too much below 110 km.

MR. ZIMMERMAN. I might add that this is only for ambient electron temperatures.

DR. ELFORD. They cool pretty quickly.

DR. PETERSON. Isn't there another problem here that the things are not really formed at all along a mathematical line, but that there is an initial size here?

DR. CHAMPION. Well, the solution isn't very different. We have done various solutions with chemicals, but the final result is the same after a long enough time.

DR. PETERSON. That is true, but as far as the meteor reflection return, the initial radius becomes quite important below 100 km.

DR. CHAMPION. This is mathematics; yes, it is quite different. This might affect the decay rates.

MR. MYERS. This would change your initial signal strength, mainly.

DR. PETERSON. Yes, that is true.

DR. BARNES. You do run into troubles when the initial radius is large compared to the wave length. Then the assumptions used to obtain the diffusion rates are violated. This happened at high altitudes and/or high frequencies.

Another problem is recombinations, but apparently it plays only a minor role in the determination of D from underdense trails.

DR. CHAMPION. You don't need to worry about that.

DR. ELFORD. Coming to the physics, the actual value is not particularly the ionic value. It is twice this.

DR. CHAMPION. Yes, the ambipolar. That would double the ionic value. That would be the electron ion temperatures.

DR. BARNES. That is another point (that may or may not be true) but it makes quite a bit of difference when you are trying to calibrate the density from the ambipolar diffusion coefficient.

THE FLOOR. What is the minimum electron density you are dealing with?

DR. PETERSON. Ten to the twelfth per meter--no, ten to the fourteenth.

DR. SOUTHWORTH. Ten to the tenth for a big pulse system.

DR. PETERSON. No, I was just taking a minimum.

DR. SOUTHWORTH. Well, the minimum is quite a bit smaller than that.

DR. CHAMPION. You see, if your electron density per volume becomes too low, then you automatically break away the ambipolar diffusion coefficient.

DR. BARNES. You mean due to the fact that you have the electrons up there already?

DR. CHAMPION. No. This means that you can have a maximum density between your electron and ion density. Essentially, when your densities are higher, the difference between them is negligible, because otherwise you get a field set up between them.

DR. ELFORD. We have looked at this at Adelaide.

DR. PETERSON. That left you in the region where electrons can't get away from the ions, so that they stay together even when the magnetic field slows down.

DR. ELFORD. I think the ions help the electrons.

DR. BARNES. Maybe we will have to go to 110 km or higher as a lower limit before we start getting a significant difference between the electron and the ion movements.

DR. ELFORD. It's the glue worked the other way around.

DR. BARNES. Are there v more comments? Well, Dr. Champion, I didn't understand all of your argument there. There is one question I would like to pose, and that is whether the electron concentration that exists in the ionosphere should be taken into account. The equations assume that you are in a neutral atmosphere and the fact is that you are not in a neutral atmosphere, but you do

already have a background of charged particles which will change the form of the equation. Is the electron concentration of the trails that we are looking at with our sets so great that we don't have to consider this background?

DR. CHAMPION. I think it is.

MR. ZIMMERMAN. You are asking for the diffusion of one type of piece or particle into a multi-component background gas. This equation is fairly well worked out. It is very nicely done, and for the most part you will find that the dominant species, the one with the greatest concentration, is the only one that you have to worry about in this case, which is in the neutral atmosphere in the region you are considering. You will just have to consider two part equations in your case.

DR. BARNES. Thank you, Sam. To return to the previous equation there is a solution for this differential equation. For a particular solution, you put it back into the equation, solve for the boundary conditions, and come out with

$$N(r, t) = \frac{q}{\pi(4Dt + r_0^2)} \epsilon^{-[r^2/(Dt+r_0^2)]} .$$

This raises another mathematical point. You have one solution to a differential equation; this one happens to fit the model, and in most cases in physics when you find any solution that fits the model, then you can use this particular solution. But there might be other solutions, and they might work. The mathematicians can't tell us whether there are other usable solutions or not, but at least we have one solution which seems to work, or which we are using.

Again, we have this q plaguing us.

DR. PETERSON. But that q doesn't bother you now, does it?

DR. BARNES. Why not?

DR. PETERSON. I assume here that you take the q at the point where you are interested.

DR. BARNES. With this model you are assuming that you have got a line.

DR. PETERSON. You are assuming that it is a constant along the line?

DR. BARNES. Yes. Then you get your ambipolar diffusion so that you are diffusing only radially.

DR. PETERSON. I see.

DR. BARNES. What happens is that if you have variations in q so that you have a blob, then it is going to diffuse in not just two directions, not just in directions perpendicular to the trail, but in all three directions.

DR. ELFORD. Doesn't this last expression refer to a small segment on the trail?

DR. BARNES. Right.

MR. ZIMMERMAN. It is based on this solution as a symmetrical case involved at any point along the cylinder.

DR. BARNES. Making the proper substitutions we obtain the ratio of the power received at some time t after the initial time 0.

$$\frac{P_r(t)}{P_r(0)} = \exp\ [-32\ \pi^2 Dt/\lambda^2] \times [-8\pi^2 r_0^2/\lambda^2]\ .$$

I couldn't see that there were any questions in the derivation of this, except the one that I brought out previously (which Sam has clarified) that you are diffusing into a medium where there are already electrons and ions.

DR. ELFORD. Are you still limiting yourself to a segment, or is this now the amplitude from the whole of the trail?

DR. BARNES. From the whole of the trail.

DR. ELFORD. If this is from the whole of the trail, you have a greater assumption. D is the same all the way along the trail.

DR. PETERSON. And also q .

DR. BARNES. You have to consider where the primary signal comes from, the first Fresnel zone, and this is what you are looking at.

DR. SOUTHWORTH. I carried out a large number of machine computations of Fresnel patterns, putting in diffusion to see what that would do to Fresnel oscillations. And I studied the decay of Cornu spiral and found that I absolutely could not resolve the difference between taking the diffusion from the difference at the first Fresnel zone unless I had very irregular diffusion of electrons along the trail.

DR. BARNES. And this is something that you could probably see by looking at the Fresnel zone.

DR. SOUTHWORTH. Yes, it is within the limits of the strange patterns I tried. The diffusion came out right. The decay corresponded to the diffusion rate at the first Fresnel zone.

DR. BARNES. What you are saying is, that if we have q fairly constant over the first Fresnel zone, then we are in some hopes of calibrating the density.

DR. FORSYTH. I think I agree, if I understand. We have done these calculations the same way you have. You can't weight it simply by the variation of diffusion rate along the trail. That is, you can't weight it much one way or the other, but you can weight it in our experience if you have a small fluctuation in the frequency.

DR. SOUTHWORTH. I am wondering if you had a statistical treatment of the small fluctuations. I know certainly that you could get an extreme case, and you are likely to if you make up an artificial model, and my impression was in a rapidly changing physical situation that the Fresnel zones are moving around and so forth and you are not going to have an extreme case for long.

DR. PETERSON. If you have had enough fluctuations along the Fresnel zone, you might be able to gather two or three.

DR. SOUTHWORTH. In fact, my Fresnel zones are moving along the ionization patterns anyway. I read them that way.

DR. FORSYTH. If you take the fluctuations that are there when the trail is formed, and assume that those fluctuations stay, then you will get a weighting, and this is the only way I could possibly see to explain the observed difference in decay rate for different frequencies.

DR. SOUTHWORTH. Well, I do not understand how you get those differences.

DR. FORSYTH. I think if you take the observed fluctuations, let's just talk about q, and say it is all due to the fluctuations in q.

DR. SOUTHWORTH. I think I see what you are doing. I don't understand it.

DR. PETERSON. Well, the only way he can get this is to have a few bases that are causing discrete differences at different frequencies. If it were fully statistical, as you describe, with large numbers, I guess it would then approach the uniform.

THE FLOOR. Well, obviously, it does get smaller. If it is very large, then you have the other things, and you still aren't getting fluctuations that are on the order of the size, but smaller than, the Fresnel zones. Then you are in trouble. I don't know how often it happens, because for these things you have to have a very small measurement. It has to be underdense.

DR. BARNES. Any more questions or comments?

DR. ELFORD. I don't think it is so difficult as one might think. We use a cw system. You don't need a bandwidth of 500 cycles or 600 cycles to see a Fresnel change. A kilowatt of change with a cw means you can get down to the small particles and probably measure these.

MR. ZIMMERMAN. I think you need more kilowatts. It will do a very nice job.

DR. ELLYETT. Just a few points here; you have two orders of fluctuations here; you have fluctuations of the Fresnel zone and you have fluctuations inside the Fresnel zone. On the fluctuations inside the Fresnel zone, the smaller ones, we did a lot of work on that and found it was a Gaussian distribution on the inside of the Fresnel zone.

THE FLOOR. I would like to make one comment about the diffusion in the equation. It is comforting to see that you do occasionally get exponential decay.

DR. BARNES. I have some that I will show you later.

DR. ELFORD. We have sorted thousands of these, something like two and a half thousand by hand, and after carefully checking that they were underdense, we found that half of these showed good exponential decay, but this was well after the Fresnel zones were formed.

DR. BARNES. In order to use this method to obtain densities, some selection process has to be employed to eliminate the trails which give "bad" density values. The question now is, can something be done along this line?

DR. ELFORD. Let me just say that if you do this, when you measure the height independently as we do, and you plot the diffusion coefficient with height, you get the scatter diagram where the scatter is the order of---sky-high.

DR. SOUTHWORTH. This is exactly the point! The measurements I am talking about are with linear exponential decay. You take measurements on the same trail and you get different answers.

DR. PETERSON. Let's ask again, are the values all taken in a short period of time, or are they taken over days or so?

MR. ZIMMERMAN. Well, days, certainly.

DR. PETERSON. Well, won't D really change?

MR. ZIMMERMAN. D can't change on one trail, a single trail.

DR. PETERSON. No, no, in your case that is right, but in these other cases it could change.

THE FLOOR. There are many trails that do have good exponential decay.

DR. CHAMPION. We talked about diffusion a moment ago. We haven't gotten as far as density. There is some more information in between.

DR. BARNES. Yes. Let us proceed with the derivation unless there are some more comments at this point.

If you measure the power in dbm, the formula you get for calculating the ambipolar diffusion coefficient is

$$D = \frac{-\lambda^2}{320M\pi^2} \frac{\Delta dbm}{\Delta t} .$$

Now I will discuss our method of obtaining D. Figure 2 shows the digital data obtained from a real trail. Each line represents the data obtained from one pulse of which there are 488 per second. Look-up tables are used to convert the EL.A, EL.B, and Video values to dbm. The use of look-up tables allows us to take into account the nonlinearities in the receiver systems. If a receiver or amplifier is modified, replaced, or readjusted, we recalibrate and produce a new look-up table.

Figure 3 is a plot of the decay values after they have been converted to dbm. The second line across the top is the information for each trail, just as it appeared on the listing of the cards I posted here yesterday. The third line and below is part of the graph. Signal strength runs from -110 dbm at the left to -60 dbm at the right.

METEOR TRAIL OBSERVATIONS
DIRECTION N TIME 207

RANGE	EL.A	EL.B	DA	DB	VIDEO
123	0	6	1	1	48
123	86	78	1	1	49
123	0	6	1	1	50
123	85	80	1	1	51
123	0	6	1	1	50
123	80	74	1	1	50
117	0	6	1	1	11
123	91	89	1	1	50
123	0	6	1	1	50
123	91	91	1	1	50
124	0	6	0	1	48
123	84	86	0	1	47
123	0	6	0	1	48
123	96	89	0	1	49
123	0	6	0	1	49
123	74	73	0	1	48
123	0	6	0	1	51
123	88	86	0	1	51
124	0	6	0	1	52
123	83	80	0	1	49
123	0	6	0	1	50
123	84	77	0	1	50
123	0	6	0	1	50
124	84	82	0	1	47
123	0	6	0	1	48
123	89	83	0	1	50
123	0	6	0	1	50
123	85	85	0	1	49

Figure 2. Flexowriter
Printout of Real Trail, July
1966 (A.A. Barnes)

Fifty-seven of the 246 pulses are shown in this figure. Because of the spacing each value can be plotted only to the nearest 0.5 dbm. The number plotted is the nearest 0.1 dbm, which is just beyond the accuracy of the look-up table which generally has steps of 0.25 dbm. This was the only trail with a good, smooth decay rate and Fresnel pattern out of something like 130 recorded trails. One can easily see the Fresnel pattern, and by taking the slope I got a density-height of 86 km, which is the same height calculated from the range and elevation angle. I have been using plots like these to develop methods for selecting those trails that should give good density-heights.

MR. NOWAK. Question. You mentioned that you had 129 out of 130 trails that gave you a disagreement between measured and decay height. Now, how big was this disagreement?

DR. BARNES. No, you misinterpreted me. There were approximately 130 trails. There were only a few that I could use to calculate the decay. The rest of them gave me wind data of some sort. I think that out of this run there were approximately three or four for which I would even consider trying to obtain the density.
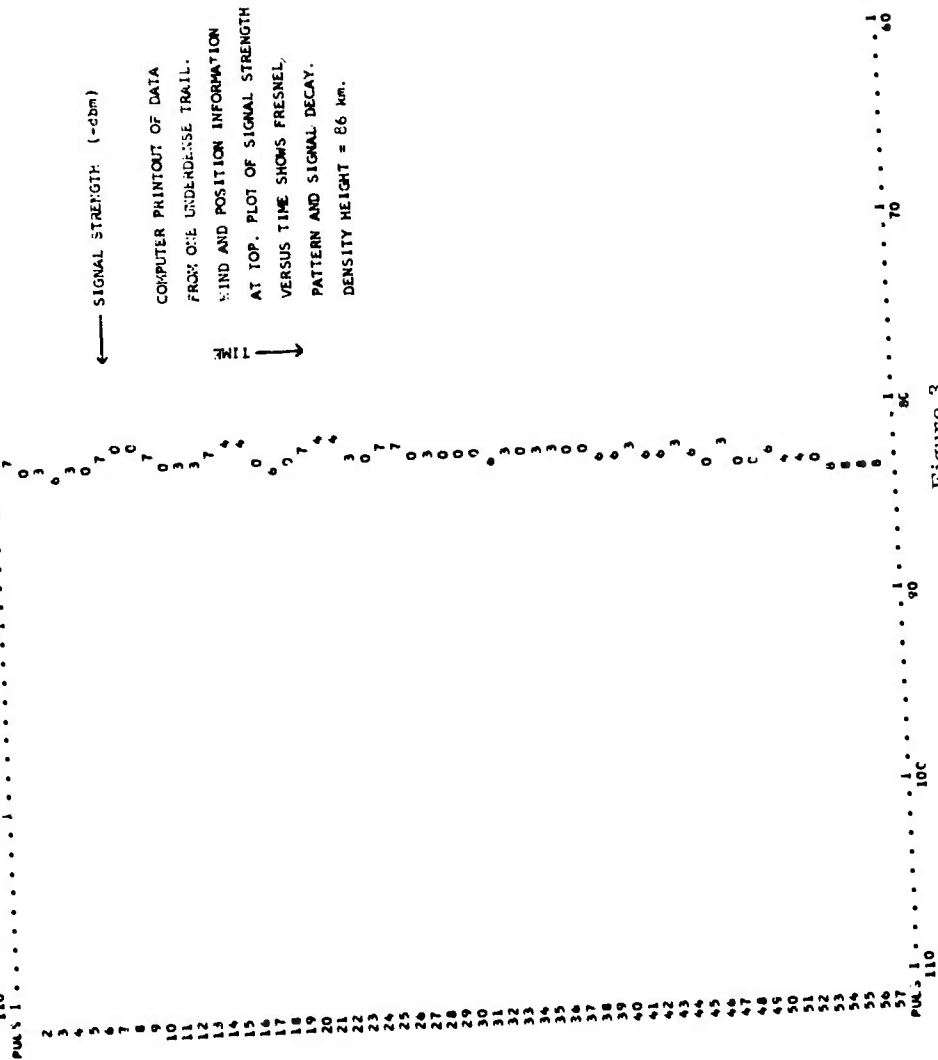
MR. NOWAK. What kind of discrepancy did you get?

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Figure 3.

DR. BARNES. Well, first, I looked for a slope, a straight slope, and this got me down to three or four. Then when I started to look for those with Fresnel patterns, I got only this one. So, the selection process that I had set up was to look for Fresnel patterns, (which I was planning to program into the machine). I think now, after hearing the comments this morning, that it might not be worth doing. Looking at the Fresnel pattern and the slope (with no break anyplace until I lost the signal) I only got this one trail. It had the same density-height and radar-height but after the previous comments, why I think I was fortunate.

DR. PETERSON. If we did that kind of selection, you would probably pick a height in which there was no wind shear, because the shear might make breaks in the slope.

MR. MYERS. You just use the slope until you get a break.

DR. PETERSON. Well, I would think that you could use it until you got a break.

DR. BARNES. No, you can't do this, because then you can't distinguish between your overdense and your underdense trails, because your overdense trail will have a break in it. With the overdense trail you will see the Fresnel pattern, and a small decay, which, at a certain point, will change abruptly to a larger decay. In order to use this latter decay, you have to know the electron density. I can't do this because I don't know my antenna patterns well enough.

DR. PETERSON. I would be worried because it might bias you away from the sharp shear when reading the slope.

DR. SOUTHWORTH. How do you know what your shear reading is?

DR. PETERSON. You don't, but there is more likelihood of break.

MR. MYERS. If you are trying to collect chronological data, then you should collect things that you could average. Shears might be a good thing to stay away from, anyway.

DR. BARNES. But your underdense trails are only going to last a few tenths of a second, so the question arises whether a trail has been distorted enough to do this. You generally find that an underdense trail has gone into the noise before you get to a half a second.

DR. SOUTHWORTH. Well, the reflection points move at once, of course, and there is no delay on this.

DR. BARNES. What I am saying is that, in order to distort the trail, you need more than a tenth of a second.

DR. SOUTHWORTH. You probably won't get more than one reflection point.

DR. BARNES. Right, yes, this is the way I see it.

DR. PETERSON. But it will move along the trail, so we will get a slight change in the D.

DR. MILLMAN. A shear zone will move five to ten meters in a tenth of a second.

DR. SOUTHWORTH. It will move more than that.

DR. BARNES. But look at the length of the Fresnel zone that you are working in. You see, your Fresnel zone is on the order of a kilometer, so you move along a few meters, and this is really insignificant.

DR. SOUTHWORTH. The reflection point moves characteristically at 10 to 20 percent of the meteor velocity. You have gone over that.

MR. MÜLLER. We got on the order of 2 km/sec.

DR. BARNES. Even assume 2 km/sec along the trail; the Fresnel zone is hundreds of meters, so it takes tenths of a second to move beyond the Fresnel zone and your trails generally don't last much longer. Generally you are still within the initial Fresnel zone where your energy was coming from.

DR. SOUTHWORTH. No, you have averaged over this.

MR. MÜLLER. Yes.

DR. ELFORD. As Southworth pointed out, he has done this. He has the artificial distribution. It doesn't make much difference.

DR. SOUTHWORTH. I did not have a moving distribution of electrons. What I said was that taking a realistic variation of diffusion coefficient with height along the trail and putting in rather irregular ionization curves, I still came out with this small percent of variation from the Fresnel zone. I was certainly not considering the shift of the wind shear.

DR. BARNES. I think there is a simple way around this problem since you can detect those that have a wind shear. From what was said yesterday, we can detect a wind shear from the Doppler changes. So, you can use this as a criterion to throw out those trails.

Do you often have trails distorted by wind shears?

DR. PETERSON. The answer is you just don't know this.

DR. ROPER. The Doppler changes often.

DR. ELFORD. I think you have to consider wind shear all the time.

MR. MÜLLER. There is an attractive aspect here in connection with telling the sign of the gradient. We mentioned yesterday what I call u' , the gradient which cannot be arrived at in terms of sign, but if you have a change in slope, then you know if your reflection point is moving up or down.

THE FLOOR. A change in decay?

MR. MÜLLER. Certainly the decay rates change throughout.

DR. BARNES. If you have a "theoretical" trail.

MR. MÜLLER. Yes.

DR. BARNES. Then the question arises whether you could actually see the change in the rate because your trail doesn't last very long. In order to obtain the

slope you have got to have a number of pulses, and you do not have these pulses. Hence it is a question of whether you distinguish two distinct slopes. There is also the question about the diffusion coefficient. Has it changed enough?

MR. MÜLLER. You need about 2 km, ideally, to tell. This is what we found out. So, if your reflection point moves about 2 km, then you can definitely tell if it is moving up or down, whereas if it is a little bit less, it is doubtful. Usually the rate of change in height is less. This is not along the trail, you see, this is actual height I am talking about.

DR. SOUTHWORTH. We are not moving anything physical for 2 km.

MR. MÜLLER. There are two different things; the speed of the reflection point along the trail and the actual height intervals throughout the travel. If that is less, say, than 2 km, then it is not very fair to derive the height from the change.

DR. SOUTHWORTH. I would suspect that 2 km is something that you do not often reach on underdense trails.

MR. MÜLLER. No, you only have this very infrequently. I have observed this on certain echoes.

DR. PETERSON. You should begin to get uncertainty figures, 0.5 to 1 km, and what you mean by D at a given height.

DR. SOUTHWORTH. This is essentially the point I was going to make this afternoon. The real difficulty is that since you are moving to a different part of the trail, with different ionization in many cases, there is a change in the diffusion.

DR. ELFORD. This is a matter of how long you observe, if you have a shear region, and if you observed for a short time, this is going to be reasonably exponential. If you observe for an especially long time, you are going to have shear, the shear is going to move, and the reflection point is going to be further and further away and you'll have different rates, so you will need to be changing your decay slope.

DR. BARNES. That is right, but if you have an underdense trail to begin with, is it going to last this long?

DR. ELFORD. That depends upon the power of your equipment.

DR. PETERSON. And the height of your observation.

DR. BARNES. And the range.

DR. ROPER. If we just take time and measure less than a half a second, then my figures would show that on the average you have moved approximately 500 meters along the trail. I think most people would agree with this. This is very reasonable.

DR. SOUTHWORTH. The reflection point moves 10 percent of the velocity of the meteor.

DR. PETERSON. Yes, but that number agrees with his number.

MR. MÜLLER. We have results covering the whole year, and essentially it is not less than 0.67 km. This is the rate of change that we observed. We could say that this data was taken at very very close range. It depends, of course, upon the technique.

DR. SOUTHWORTH. Yes, I actually, to a large extent, have looked at the optical scale to see the very large range of shear there, and over a short range there is none, and it has extremely high winds, and the optical trail is fair.

MR. MYERS. It seems to me that all of this makes Arnold in a more and more favorable position, because he is vigorously, objectively screening. By selecting only short trails, by picking his selection points, he is going to throw away an awful lot of trails, but the ones that he has left have a chance of behaving in the same fashion anyway.

DR. SOUTHWORTH. Yes, but I am afraid that the problem is whether you can conceivably tell.

DR. PETERSON. Just for the sake of saying it, the only case where it can happen is where there aren't two blobs or two shears.

MR. MYERS. You may not have any trails left. We got one out of 130.

DR. SOUTHWORTH. You have absolutely no way of knowing which is a good trail. A good trail looks just like a bad one.

MR. MÜLLER. You are making it too hard for yourself, Dick.

MR. MYERS. I think so, too. You say that a good trail you can't tell from a bad trail if there is no difference in the signal?

DR. PETERSON. No, he says one in which the reflection point moves but will still have exponential decay.

DR. SOUTHWORTH. It will give exponential decay, a reasonable Fresnel pattern. It can be a perfect one.

DR. ROPER. What about the Doppler?

MR. MYERS. It might not be affected by Doppler.

MR. MÜLLER. Above the magnitude that we mention, you get a definite change in Doppler. It has nothing to do with the Fresnel patterns at all, so those changes would probably be ten or fifteen degrees compared with ten times 360°.

DR. SOUTHWORTH. You are looking for a long duration trail, again.

DR. ELFORD. I take it that you are using another receiving site, as we have done, to get, say, 1 or 2 km.

DR. BARNES. I would suggest looking at the change in the Doppler to get the shear.

DR. REVAH. We have checked the change in the Doppler looking at the variation of phase as a function of time. These trails on which we made the height measurements, we looked for the phase variation as a function of time and we have here an example. I have a slide (not available) showing the behavior of a Doppler

which remained constant during the life of the trail, and a sample where the Doppler varied. We sampled every ten seconds. I think the question of wind shears might be clarified by this method, but it is not a good method by which to recognize a good trail from a bad one, or a good Fresnel pattern.

DR. MILLMAN. I would welcome a clarification of the real reason for trying to determine these heights by this method. Are you trying to get heights from the diffusion?

DR. BARNES. We are trying to get the density at various heights. We obtain the height by the range and elevation, and then we want to look at the variation of density at particular height levels.

DR. MILLMAN. So your basic aim is to get the density?

DR. BARNES. That is correct.

DR. MILLMAN. From some of the remarks that were made, I thought you were trying to get height from the diffusion.

DR. BARNES. No, on my figure I show the density-height, just to show that it corresponded.

DR. MILLMAN. But you are not really interested in getting density-heights?

DR. BARNES. Correct. You have to make a number of assumptions in order to do this. You have to assume some atmosphere, and I have assumed the U.S. 1962 Standard Atmosphere, but what we are really interested in is the densities and the variations of the densities at fixed height levels.

DR. MILLMAN. Yes, but you are very happy to determine height in other ways?

DR. BARNES. Yes.

DR. PETERSON. In fact, he finds it essential.

DR. MILLMAN. Yes, because I was a little worried that you were trying to get your heights this way, too.

DR. REVAH. Is it easier to derive the density-height altitude on the assumption of geostrophic wind (that means looking only at the prevailing winds) and try to derive the density by the classical equations?

DR. BARNES. This is all very nice, but this does not help when you are trying to study the general circulation of the atmosphere where you want to look at such things as the mass transport. In this case when you use the geostrophic assumption, you have already determined the mass transport, so you get no information out of it. It is this additional information that we need in order to construct what is actually going on in the atmosphere, because the atmosphere is not always geostrophic. It is the ageostrophic component that is important in determining exactly what is happening in the general circulation of the atmosphere. This is one of the reasons we are not following this line of attack. We know that we can go this way, but we would like to know if we can do it another way.

DR. REVAH. You want the microscopic view of it?

DR. BARNES. Yes, but primarily because it is needed to construct the macroscopic view.

DR. BARNES. Are we ready to continue the discussion? Allen Peterson asked me a question that I was not able to answer, and I think he got the answer from somebody else.

DR. PETERSON. The question I was asking was what do we know : .t the density variations produced by gravity waves going through this region, and the answer I got from several of the experts at CRL was 10 to 20 percent. I thought that was a little high, but that much of a percentage for these variations in gravity waves which move through with a period of something like 10 minuts to half an hour would then throw a fluctuation on our measurements that we would interpret as a variation in height that is on the order of 0.5 to 1 km and that would then make us feel that even if we had a very accurate system that somehow they were missing the true measurement by the order of this in height. If there is indeed that much variation due to gravity waves, then the short term fluctuations in density at a given height could be on the order of our possible accuracy at the present moment. That might mean that if we improved the equipment, we might expect to get to the point where the data doesn't seem to improve. On the other hand we should interpret that as new information of use to the meteorologist instead of the fact that our equipment wasn't getting any better.

All I wanted to do was raise the question of possible effect of these gravity waves on the diffusion coefficient and see if we can at least make some suggestions as to what we should do to understand it better.

Nobody denies these numbers?

DR. SOUTHWORTH. What sort of wind velocities are involved?

DR. CHAMPION. Do you mean wind velocities or the velocities of the gravity waves?

DR. PETERSON. The gravity waves are something on the order of 500 m/sec.

DR. SOUTHWORTH. Not the way that Peter Forsyth speaks. How fast did the particles of waves move to go through?

DR. REVAH. Are we talking about the wind speed?

DR. SOUTHWORTH. Yes, wind speed due to gravity waves.

DR. REVAH. That would be 1 to 20 m/sec and higher.

DR. CHAMPION. Some of these gravity waves have very short winds. They don't have an associated wind system oscillation.

DR. SOUTHWORTH. Now, that would be fascinating!

DR. BARNES. In order to have it affect the wind measurement, it would have to act over the first Fresnel zone as a whole.

MR. ZIMMERMAN. If you are asking for the localized velocities, I would at the most expect the same velocities as in the winds created by tidal loss.

DR. SOUTHWORTH. I had some such impression. This is one of the things that we want to look at again.

DR. BARNES. Why doesn't it show up in the data, then?

DR. PETERSON. Well, I think it does; I think it shows up in your data as a spread, but we don't have continuous enough data to show it up as a gravity wave.

MR. MÜLLER. I shall show you some slides tomorrow where it shows up in the time scales. There are no periods of less than about 30 minutes.

DR. PETERSON. On a theoretical basis you can get waves up through the ionosphere in less than ten minutes.

MR. MÜLLER. Yes, well, this is the order of magnitude, but it means that if the phenomena are periodic, then you might as well average.

DR. ROPER. We have taken all of our wind measurements and extracted the tidal winds from them and looked at the magnitude of the residuals. These vary from 50 to 60 m/sec, the same order of magnitude.

DR. PETERSON. I guess the point I wanted to make was that in this decay rate business, this same kind of range fluctuation ought to be reflected, so that we couldn't hope to have D constant at a given height to better than this sort of 10 percent number, unless we do it on a very short-term basis.

The Australian group at Sidney has certainly made a lot of these observations with many of these gravity waves moving along. They show them in sporadic E, also.

DR. BARNES. Any more comments or questions?

DR. FORSYTH. I would like to make a comment and ask a question about it in regard to the measurements of the decay rate. As long as you average, you always come out with the right answer. That is, statistically this is a good thing to do. Certainly with the multi-frequency measurements if you average enough you do get the same answer from more frequencies. Is this the impression that everyone else has that averaging is a perfectly valid thing to do in this decay rate?

DR. ELFORD. Could I ask a question? What do you mean by averaging?

DR. FORSYTH. Averaging at a different height. The question is how do you average? If you average the decay constant that you derive at a given height, is this a valid answer? It is relatively easy with the multi-frequency work.

MR. NOWAK. What is the dispersion of the result for deviation at a given height?

DR. FORSYTH. I can't remember.

DR. SOUTHWORTH. I have data on this. It comes up to correspond to 3 or 4 km height. I think the averaging is perfectly valid as long as you are careful that

you are averaging decay rate and not its inverse or something like that. You have to be very careful about probable errors and systematic errors due to large standard deviations and all that.

MR. ZIMMERMAN. Your error is on the order of a scale height, plus or minus; and that would mean that your diffusion coefficient is measured on the order of a magnitude, plus or minus.

DR. SOUTHWORTH. It isn't quite that big.

DR. ELFORD. Maybe on the order of three rather than ten.

MR. ZIMMERMAN. Can you believe in averaging of this when you have an order of magnitude spread on diffusion?

DR. ELFORD. Does this mean that if you average tomorrow and if you average today, you get the same answer?

MR. ZIMMERMAN. I don't know. I think I do know the answer for the multiple frequency measurements. There you come out with the same answer and that means you are right.

DR. PETERSON. I was going to answer your question that the average is probably fine as long as you really don't want variation.

DR. ELFORD. One other fact comes to mind: having made this effort, if you then try to draw an average line through the scatter diagram, it has the wrong slope if you are trying to relate it to density. However, we haven't gotten to density yet.

DR. BARNES. Shall we then go to the question of density versus decay height? Could we have the next slide, please? (Figure 4). We have the log of D, the diffusion coefficient given in cm^2/sec along the X axis. The height is given in kilometers on the Y axis. This solid straight line is that which Greenhow derived from his data, and the equation for the straight line is given at the top.

What is the relationship between the diffusion coefficient and density? The formula that I found to look the most satisfying is given in the center. There were a number of equations in the literature, and some of them were of the Greenhow form, but theoretically they are not very satisfying if you follow the derivations through. After looking at all of the different equations that had been derived, the lower equation is the one that I decided to use. This is the one that Rice, I believe, derived.

Now, if you take the standard atmosphere and insert the proper values at the different heights, then you obtain for the 1952 Standard Atmosphere the two points shown. For our work we used the 1962 United States atmosphere and the lower equation to give the dashed line. The agreement of the curves is fairly good below 90 km, and there seems to be quite a departure as you move higher up into the atmosphere.

DR. MILLMAN. This is assuming the density of the standard atmosphere in obtaining D?
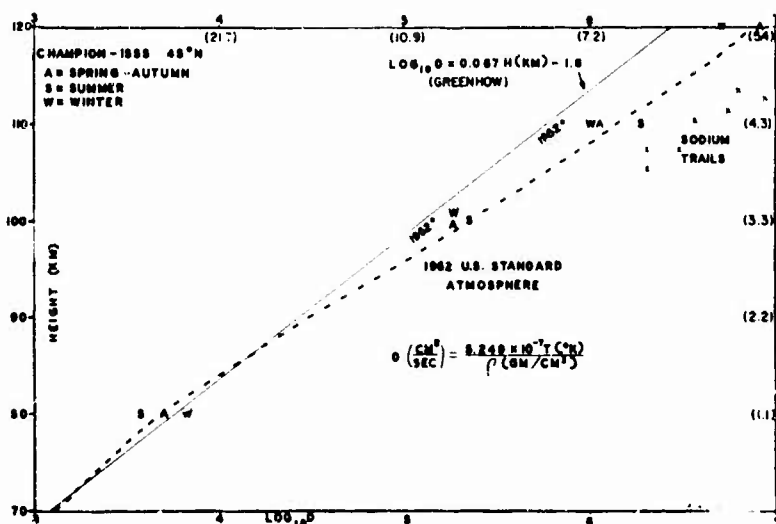
Figure 4. Molecular Diffusion as a Function of Height

DR. BARNES. That is correct, by the formula in the lower center of Figure 4.

DR. ELFORD. Now just for a moment, I am quite happy about that formula. The observations of ionic diffusion are very well-known to all from laboratory measurements. The value of D doesn't vary very much from one ion to another, and I think the relationship between the value of D, the density and the temperature, are established from laboratory and theoretical work. I am happy about that work.

DR. PETERSON. How about the constant?

DR. BARNES. The constant is derived. This constant, 5.249×10^{-7}, seems to me came from experiments in the laboratory, is that correct?

DR. ELFORD. Yes, that is right.

MR. MÜLLER. There is a term in there concerning the collision cross-section?

DR. BARNES. That is correct.

MR. ZIMMERMAN. May I ask a question on the equation? If I remember, Greenhow utilized the equations derived by Chapman and Cowling, and that is another history for molecular diffusion, and this is the one that has been applied by many people, which gives you a temperature to one-half dependence over number density times mass density.

DR. BARNES. That is right. You get temperature to the one-half power.

MR. ZIMMERMAN. I am curious as to how you came to the selection of this, since I read about this relationship once and am confused as to how he arrived at this solution.

DR. BARNES. Well, I will have to admit that I looked at the other one and I was confused as to how it was derived.

MR. ZIMMERMAN. Well, that is very simple, on a diffusion coefficient basis, the mean velocity times one-third value. It is very simply from kinetic theory.

DR. ELFORD. This one (in Figure 4) isn't based on derivations. This is in fact taken from L. G. Huxley, who is an electron-ion diffusion man, and all his work is based on mean free path analysis. He did this work about the same time that Kaiser published his work. They worked independently. Huxley did it in terms of ambipolar diffusion problem.

DR. CHAMPION. It all depends upon how you read Huxley's version, and there is more than one possible dependence that has been worked out.

DR. ELFORD. It doesn't make much difference in the end result.

DR. BARNES. Actually at one time I plotted up all of the various formulas that existed in the literature, and put them all on the same chart---.

MR. ZIMMERMAN. In order to hav. the effect of the mean free path and the ionic component, and the function of temperature, this implies that the electron-ion collision frequency is large compared to the neutral collision frequency, am I correct?

DR. ELFORD. No.

MR. ZIMMERMAN. Yes, because if the electron-ion frequency is down, then you have single particle collisions with the neutral, where the electron only sees the neutral on the independent electron collision and you go to Chapman's relation.

DR. ROPER. No, this essentially is an ion diffusion coefficient. This is ambipolar and this is when your ion diffusion coefficient is neutral.

DR. ELFORD. You have forgotten about the electron altogether.

DR. BARNES. Well, as I said, the dashed line is from the 1962 Standard Atmosphere. Next I wanted to look at seasonal variations, so I took some values by Ken Champion for winter, autumn, spring, and summer for 45 degrees north. A is for spring or autumn, S is summer, W is winter. You see the points fall fairly close to the United States Standard Atmosphere for 1962. There is good agreement at 80 km. The 90 km values were right in with everything else. Everything seemed to cross about 90 km. We are not taking into account the seasonal variations in our data reduction at this time because this is something that has come out recently. If we want it, we use the dashed line to get the density-height. Assuming the 1962 Standard Atmosphere, we find out what the diffusion coefficient is; plug it in, and then we come back with the density-height. This is the standard atmosphere density-height that I mentioned previously.

DR. CHAMPION. If I may make a comment. If I remember correctly in this altitude region where there are diffusion coefficients from the chemical cloud, the

observed diffusion coefficients are not the molecular values, but the turbulent co-efficients which are something like an order of magnitude higher. Then, at some-what higher altitudes, when you don't have turbulence, you certainly have the molecular diffusion coefficients.

MR. ZIMMERMAN. That would depend upon which experiments you are refer-ring to in this case.

DR. BARNES. The sodium.

MR. ZIMMERMAN. The sodium trails that were reported by GCA people?

DR. BARNES. That is correct.

MR. ZIMMERMAN. That is what I thought. Their technique of measurement was very questionable at this low altitude region, below 135 km.

DR. BARNES. Yes. These are 3 to 7 sec averages, too. Also these are not ambipolar, but are diffusion in all directions.

MR. ZIMMERMAN. These are molecular diffusions? This isn't neutral? In this case, no one really understands why there is some hint that this is some tur-bulent form of diffusion that looks like molecular, but it is created in the method of deposition. These are very high temperatures, very high velocities to get the sodium trails. Malcolm MacLeod came out with a molecular diffusion coefficient based on TMA trails which are relatively low energy trails, and they showed that the standard 1962 Atmosphere fits very nicely with their experimental results. These were at altitudes above 106 km.

DR. BARNES. Now, how do they get their diffusion?

MR. ZIMMERMAN. They use the same method; they measured the dispersion. This is a neutral particle.

THE FLOOR. Do they measure what time scale they are using?

DR. BARNES. You see, time scale enters into this, because you have turbu-lence.

MR. ZIMMERMAN. Not above 106 km; that is the point. There the time scale is on the order of 300 or 400 sec. They measured it, and it follows very beautifully the theory for molecular diffusion above 106 km, and time scale has nothing to do with their measurements. The sodium trails introduced the turbu-lence into the atmosphere, which is created by a turbulent jet, and it looks and has the appearance of a molecular diffusion with an accelerated diffusion coefficient.

DR. BARNES. That is what I was going to say. This is turbulence, and this is the reason that the sodium trail points on my Figure 4 are further to the right. That was my interpretation.

MR. ZIMMERMAN. That was turbulence most likely created by the jet. It is not an atmospheric turbulence.

DR. BARNES. My next slide (Figure 5) shows the height, 80 up to 120 km, and this is the mean seasonal deviation profiles plotted as percent departures
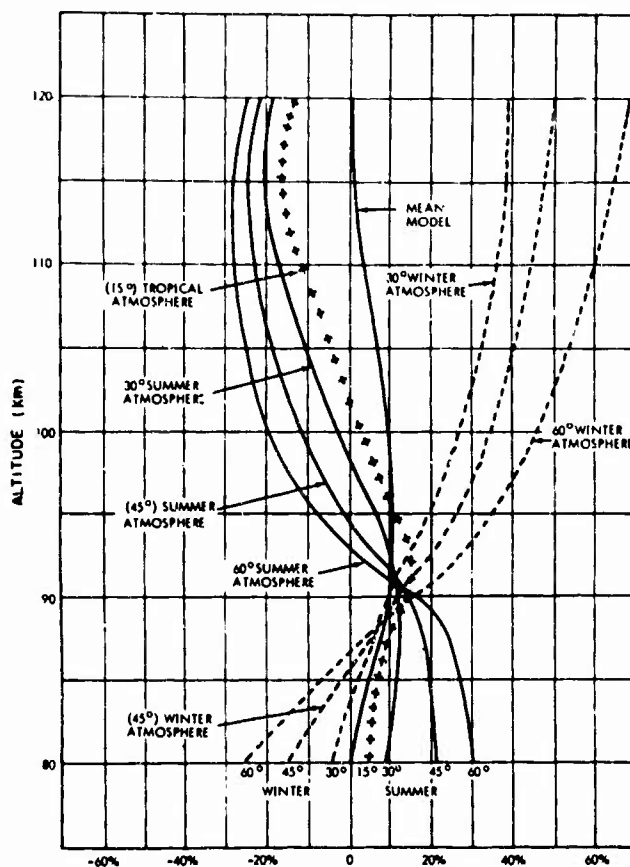
Figure 5. Mean Seasonal Density Profiles
(Plotted as Percent Departure from Standard)

from standard. The mean model is the 1962 Standard Atmosphere. There seems to be a crossover point at 91 km. That is where Al Cole believes the crossover occurs. Then you can see there are large variations depending on how far you are from the equator, and what season you are in. It is variations like this that the meteorologist is interested in obtaining and studying. So this is one of the reasons why we want to continue with the density measurements, even though it looks very doubtful whether we will get anything out of the individual trail measurement. By taking averages, we can get points to better define the standard atmosphere at the various altitudes. Al, do you have any comments on it?

MR. COLE. Just that that is Dr. Champion's drawing and it was taken from actual data.

DR. PETERSON. What is zero on there?

DR. CHAMPION. Standard Atmosphere.

DR. BARNES. These are departures from that standard atmosphere.

DR. CHAMPION. No, these are averages also of all the available data at the time that we did that.

DR. PETERSON. It looks like the mean has certainly clustered near 91 km.

DR. CHAMPION. This was recognized some time ago.

DR. BARNES. I had an option to take this one or the theoretical one, and I decided that this one was better since it contains actual data.

DR. CHAMPION. I am sure that in a year or so we will want to revise that. That is just a first attempt to put in the seasonal and latitude variations. When we get more data and better data, I am sure that we will want to revise it, although the general principle is right.

DR. ELFORD. That crossover at 90 is very interesting, because that is where we get the maximum meteor data. We have compared decay rates on annual basis and we didn't find any decay variation on the decaying rate on an annual basis. Now, maybe that was just because that was a crossover point.

THE FLOOR. Groves shows the same crossover.

DR. ELFORD. Good, fine. Maybe this is why we didn't get data variations.

DR. BARNES. Al did considerable work on it, and the density does seem to remain constant.

DR. ELFORD. That doesn't prove, of course, that there isn't in fact an annual variation. Maybe it is coincident, Al, but this may be the explanation.

DR. BARNES. I think it probably shows that if there is one, it is probably small at this particular point. Are there any more questions?

Now, that is the end of my presentation. There are a number of questions that are still plaguing people, so why don't we just discuss them now for the next 15 minutes.

DR. ELFORD. Right at the top of that second to last slide (Figure 4), you had an equation that you attributed to Greenhow.

DR. BARNES. That is correct.

DR. ELFORD. I think that now, in order to get the record straight, we should realize what Greenhow was doing. Unfortunately he is not here to tell us what he was doing, but in his work, he was trying to find some way of relating heights to winds. So, he carried out this experiment where he measured heights using two antennas, at two different heights above the ground, and decay rates. If you look at his paper, you find that he got a very large scatter in his decay rate. He measured his heights quite accurately, to the order of 2 km. Then he said, I am only interested in a statistical basis for my winds, so he produced some sort of an expression relating heights to diffusion coefficients. Now people look at this expression without realizing that there are large variations, indeed. That is a very important point to keep in mind.

DR. BARNES. Not only that, but he then turned around and obtained wind data using the D to get his heights.

DR. ELFORD. That was quite valid, as long as he had sufficient data at a given D. It would tend to smooth out the wind data a bit.

DR. PETERSON. Those variations are so big at certain heights that it is going to give a mean value that is probably not representative for that density-height.

MR. MYERS. It is also going to assign to the winds a bias.

DR. PETERSON. He should at least have measured D for the summer and D for the winter and then used those in the right season.

DR. ELFORD. I think you will find the wind systematically smooth.

MR. MYERS. But he would assign them to the wrong height.

DR. ELFORD. I see what you mean.

DR. PETERSON. If gravity waves make a variation almost equivalent to the annual variation, then you would have to worry about them, but the gravity wave variations presumably are smaller than the annual variation.

DR. ELFORD. The second point; when you look at Greenhow's scatter diagram, and you draw his main curve, if you take the expression that you have there at the top of Figure 4, it has the wrong slope for the diffusion as a function of height in terms of the ambient density. It has got the wrong slope; it varies too closely with altitude. We have tried this out at Adelaide on 2,000 decays, and we confirmed exactly the slope that Greenhow got. It is the wrong one, it doesn't go the right way with density. This is just taking the expression D and comparing that with the observations where we measured the height and the diffusion coefficients.

DR. SOUTHWORTH. This is a real problem with the statistical treatment of that data. It is very nasty data to work with, because there are a lot of things missing. I was going to tell you one of them this afternoon.

DR. BARNES. The reason I threw out Greenhow's equation was because, mathematically, it was not satisfactory and unfortunately or fortunately I was trained as a mathematician.

DR. ELFORD. I agree with your comment, we had a go with that in our paper. A colleague of mine named Murray and I (Planet. Space Sci. (1959) p. 125) measured the height (to within 2 km) and the diffusion coefficient, and we got the slope, which is too low. If you take Greenhow's data and analyze it in what we think is the correct way, you get a slight degree of error. It wasn't the slope that Greenhow got originally. Maybe that is where I confused you. Can I draw something on the board which would just point this out?

DR. BARNES. All right.

DR. ELFORD. We plotted height versus log D. We got a scatter diagram with points all over the place. All right?

Now, depending upon which you consider the independent variable, you get two slopes. One was Greenhow's, which we think is wrong, and one is the Adelaide, which we think is right.

DR. PETERSON. How far are these off?

DR. ELFORD. When you do Gree ow the other way, they come down.

DR. PETERSON. Are they more off than the other way around?

DR. ELFORD. Greenhow roughly has the right slope.

MR. MYERS. Does yours give ne right slope?

DR. ELFORD. In one way.

MR. ZIMMERMAN. Upon what do you base your preference of your slope over Greenhow's?

DR. ELFORD. We can measure the height accurately. Greenhow had effectively no error in the height, say 1 km, or all that scatter was in the observed diffusion coefficient.

DR. PETERSON. You now leave more doubt in my mind. You are saying that though you measured it accurately, it isn't right?

DR. ELFORD. That is right. That is why I don't have any faith in the relationship between decay rate and density.

DR. PETERSON. What is wrong?

DR. ELFORD. Well, there is one thing that I think contributes to the problem and we in Adelaide investigated this in some detail. We found a strong correlation between the decay rate and the atmospheric speed, if you just take the total speed of the atmosphere when you made the atmospheric observations. Now, it is a strong correlation, but unfortunately we couldn't unravel the one to one correspondence. There were some other physical phenomena which related motion in the atmosphere to the decay rate.

DR. BARNES. Well, people have suggested that with higher winds you have higher shears. If the shear is related to the diffusion, then the decay rate and the speed would be related.

DR. PETERSON. What is the relationship between the wind speed and the density?

DR. CHAMPION. Well, one possibility is the high wind speed, and then you have higher shears and this might give you more turbulence.

DR. ELFORD. I think this could give you quite a different effect if you are pushing an ionized mass across the magnetic field, because this is going to create some sort of distortion, because you have the positive-negative ions. This could affect the diffusion coefficient.

THE FLOOR. What altitude are you talking about?

DR. ELFORD. 95 km.

THE FLOOR. I think sporadic E would show that.

MR. MÜLLER. I think you have to consider this, that there has to be a very strong coupling of the trail orientation with the lines of force.

DR. PETERSON. This is experimentally showing the effect, anyway.

DR. BARNES. Are there any more comments?

DR. PETERSON. You know, Graham's comments are a little different from the previous ones this morning. He suggests that you aren't really measuring the density.

DR. BARNES. Maybe we are measuring the wind speed?

DR. PETERSON. Or you might be mea ring something else.

DR. BARNES. Yes, Sam.

MR. ZIMMERMAN. 1 would like to rej or on some recent measurements done by Jacques Beaumont in the Sahara. He had some very small thin sodium trails which he was using to study turbulence at about 100 km. He did make the announcement that some of these trails below 100 km, or 105, show no effect of turbulence at all. For people who were doing studies on long time duration trails this could have a profound effect on the diffusion coefficients that you measure. There is no turbulence that you should see, no change above the molecular diffusion coefficient to perhaps as low as 80 km.

In reanalyzing some of Bob Roper's data, which he very very politely supplied me, 1 have come across something which seems to reinforce this assumption.

In some of the data for the month of March, there appeared to be no inertial subrange turbulence. Of course, this is questionable, because the result implies that you are measuring on a meteor trail a distance which is smaller than a Fresnel zone. It is questionable about that. But, those were with a substantial length of inertial subrange, which showed very definite values, which you can use to derive the rate of this dissipation.

In this reduced data, there appeared to be evidence which perhaps would substantiate some of Beaumont's statements; no turbulence in the 95 km region.

DR. REVAH. By which means do you measure the velocities, by meteor trails?

MR. ZIMMERMAN. This was a meteor trail. You have seen some of the data already today.

THE FLOOR. Would you tie this together; I am missing some of the significance of this.

MR. ZIMMERMAN. Well, the significance is that for people who are doing meteor studies, the assumption has been made that turbulence was always present there below 105 km.

DR. ROPER. A point should be made here, Sam, that in measuring the ambipolar diffusion, in the first 0.2 sec you are not getting into turbulence.

MR. ZIMMERMAN. That is the point that I raised. It is only for those who are doing studies on long time duration meteor trails. This would be the pertinent point.

DR. ROPER. This is certainly pertinent in terms of long duration meteor trails, but all of the decay studies are on short duration trails.

MR. ZIMMERMAN. However, if this is true, there may be another method of looking at some of the long time duration trails which may show the same sort of decay rate, on the decaying portion, as the smaller concentration. It seems to be suggested in this data.

DR. CHAMPION. It may be correct, but a lot of people don't believe it.

DR. BARNES. So, where are we?

MR. ZIMMERMAN. Well, that just gives you something else to think about.

DR. BARNES. Thank you, Sam.

DR. ELFORD. Well, faced with this problem of the large scatter, we thought maybe we could do better if we measured decay rates at two points on the one meteor trail. We did this in 1961. So, effectively, what we have now are two observations very close together in height where we know the separation accurately to a few hundred meters. The separation in general being on the order of 1 or 2 km. We know the absolute height to the order of a couple of kilometers. We have therefore a right to produce a slope, which would be correct according to theory. We have looked at some hundreds of these, and I must confess that on the average they come out with the wrong slope, the one which we got from the big scatter diagram. So, even when you do it on a differential basis, you don't come out right.

DR. PETERSON. But you did get agreement with some of your averages?

DR. ELFORD. Yes.

DR. CHAMPION. I would just like to ask Graham a question. Evidently in some of Greenhow's work, he found a diurnal variation in density at 95 km of about 50 percent or more which doesn't seem to be corroborated by any other technique.

DR. ELFORD. Right, Greenhow did that and claimed that he got the right slope. We haven't been able to corroborate it.

II. Density Measurements at Havana*

Dr. R. B. Southworth
Smithsonian Astrophysical Observatory
Cambridge, Massochusetts

DR. BARNES. Gentlemen, may we resume. I am worn out after this morn-
ing. I just hope that Dick Southworth isn't worn out because Dick is now going to
talk to us about densities and density heights at the Havana system.

DR. RICHARD SOUTHWORTH. As I said, I don't have a great deal left to say.
What I am going to do here is present some of the data that we have collected on
systems that we have been operating up until now, where we don't measure height
variations very well, and we had not been measuring phase. We tried to do as
much as we possibly could with that data, so we got out what best heights we could.
One system was to measure heights by density, which I come to later, and another
was to try to get height from a rather weak piece of geometry. You will remember
that we get the radiant direction of the meteor, and the distance from the transmit-
ter, so that I know that it lies somewhere on the cylinder. I also know that just
about all of them are about the main beam of my antenna, which is about 30 degrees
wide to 10 degrees down, if I am using a single trough which we have been using at
the main site and which we had been transmitting on for earlier data. If I use the
double trough, it is about 20 degrees wide.

Now, if I pick the right sort of gradients and note that they lie upon this cyl-
inder -- essentially I picked the right azimuth for this gradient -- so the meteor

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*Ed. note: the following was reconstructed from the stenographic notes.

lies upon the cylinder and there is a rather small resultant range in height. The standard error is about six kilometers. So, we tried to do what we could with that, and Dr. Berney looked at our measurements of diffusion. We measured diffusion at every station.

The slide (Figure 1) gives a preliminary treatment of a picture made for him to present in Italy where he is working, and I hope that you will be able to see it. The diagram was not made to go on a slide.

Here I have height on a very awkward scale, 120, 102 1/2, 85, 67 1/2, coming down to 50, here, and the log of the diffusion constant picked up at a very awkward interval; and here are all the points that he had. It is points for all day. It has been separated into various hours as well. The line was also fitted through it by least squares; and I think you can see quite clearly that the line doesn't fit the
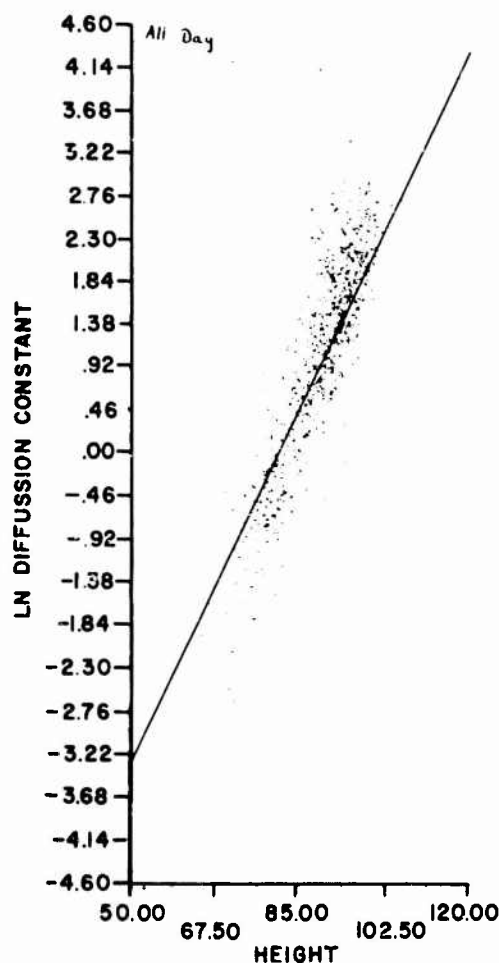

Figure 1.

distribution at all. This is one of the best examples I know of why you have trouble getting a scale height out of diffusion data.

DR. MILLMAN. I don't understand how you drew the line. By least squares?

DR. SOUTHWORTH. By least squares, it is a weighted least squares.

DR. MILLMAN. But not from those points?

DR. SOUTHWORTH. The trouble is that all the weight is in the middle points.

MR. MÜLLER. It just isn't a straight line. It shouldn't be a straight line. It is wrong to do a least squares there.

DR. MILLMAN. Even if it is a least squares, I still don't see how you get a solution from those points.

DR. SOUTHWORTH. It is a weighted least squares, and the trouble is that it is only medium grade diffusions that have weight.

DR. MILLMAN. I still don't see, because even if you weight the center points, the points at the end are still going to be affected even if they are of low weight.

DR. SOUTHWORTH. Well, you have to look at it very carefully. You will find that these points down here are all balanced by points here. You see it is just in each height.

DR. MILLMAN. Not very much. Well, okay, I will take your word for it. I don't see the least squares solution.

DR. ELFORD. Is it that we can't quite see the proper distribution at the points?

DR. SOUTHWORTH. I have the diagram here; I have it divided into different hours that you may look at. I thought the slide was going to be better than it is.

MR. ZIMMERMAN. What is the scatter?

DR. SOUTHWORTH. There is a very large scatter in diffusion.

MR. NOWAK. As I understood it, these were heights determined from your little bit approximate geometry?

DR. SOUTHWORTH. Yes.

MR. NOWAK. What would be the best estimate of the error in height by this method?

DR. SOUTHWORTH. About 6 km.

MR. NOWAK. Plus or minus?

DR. SOUTHWORTH. Plus or minus. That explains a good deal but it doesn't explain all of it.

Now, I am not going to do anything with these points as they stand this way. If there is clear variation -- there seems to be a variation through the day of these points -- I am again going to wait until I can get a better one before I do anything with them; but if Greenhow had the points, and they were the best points they had, they would surely find a diurnal variation of the density from it.

If I may have the lights, I will point out some of the difficulties I think occur with other diffusion analyses. I have a real distribution of points, height vs diffusion. Now, of course, if I put in an extra spread in height, I am going ᵔ come out with a wider line. The thing that is worse is that I really have a distribution, or should have a distribution like this. However, I can't see the top one and the bottom ones at all. The ᵔ meteors (high altitudes) diffuse too fast to go through. There aren't a great many of these (low altitude) meteors that come down this way. In any case, the decay is so small you can't measure it. These have low weights.

DR. PETERSON. You can handle that with multi-frequency. Make the low ones diffuse faster. The bigger ones at higher frequency.

DR. SOUTHWORTH. Yes, but it also means that whoever does this analysis has to realize these limitations, and I think that this has not been done at all.

DR. ELFORD. If you chop off the top 20 percent and the lower 20 percent, and do a least squares, can you change the slope very much?

DR. SOUTHWORTH. Yes, roughly by 20 or 30 percent. This is the order of the difficulties we had.

DR. MILLMAN. I must confess that I fail to understand this, because your line obviously is drawn without any respect to top or bottom. You say that your center points are the ones with the high weight. Now you say that if you chop off the top and bottom altogether, you would change it by 20 percent. The top and the bottom are now not contributing anything to the slope, so I am afraid I don't understand. I am completely lost. I am just arguing on basic principles and my knowledge of statistics. The thing just doesn't seem to fit.

DR. SOUTHWORTH. That diagram is an optical illusion.

DR. MILLMAN. Perhaps that is it.

DR. SOUTHWORTH. It is the standard problem. I take, say, an ellipsoid and perform a shear upon it, and I come out here with the axis. I make another ellipse like this. The shear has transformed that line into this, which is obviously not the main axis of the ellipse.

DR. MILLMAN. But I just couldn't detect from the diagram very much in the center there, and in the normal solution of the centerpoint. It has very little effect upon the tilt of the line, even though they are high weight. I don't see why the ends are going to have much more effect on the tilt.

DR. SOUTHWORTH. It is the optical illusion.

DR. MILLMAN. All right, I will take your word for it. You are just trying to add more confusion to what happened this morning by giving us this optical illusion. Is that it?

DR. SOUTHWORTH. This was the best available diagram that I had. Well now, the real difficulty that we find is that we compute values of the diffusion at

several stations on the same meteor, and although we are not certain at all --
within 6 km of the height of the meteor -- we know the differences in height from
one point of reflection to another quite well.

Now, the next slide (Figure 2) will remind you of
how we compute the diffusion. I take the Fresnel
pattern; I interpolate a line through the maxima,
and another line through the minima; take means
here. This is done entirely during the Fresnel
oscillations, because up until now with our film
readers, we have not measured past the Fresnel
zone, and therefore it is only the early part of

Figure 2.

the trail. I have no worry about long duration trails. We get very beautiful linear
decay.

The next slide (Figure 3) shows the distribution of the relative error of the
diffusion coefficient; that is, one over the diffusion coefficient times the standard
deviation. This is a rather small sample that I collected, but you can see that
most of it is less than six percent error. So, I have a pretty good straight line on
a large plot. However, I just don't get reasonable data from one station to the next.

DR. PETERSON. You said you didn't know the height to six kilometers. How
do you get the number like this then?

DR. SOUTHWORTH. This represents the internal error in fitting a straight
line to the logarithm of amplitude.

DR. PETERSON. Oh, you mean the exponential decay?

DR. SOUTHWORTH. Yes, this is how I get the straight line.

DR. MILLMAN. Because you do know the relative distances along the trail?

DR. PETERSON. This is from one. That is just the exponentiality.

DR. SOUTHWORTH. This is the sort of data we are getting out. You see, I
expect something of the order of 0.04, 0.03 in decimal log, which is the probable
error of the decimal log of the diffusion coefficient.
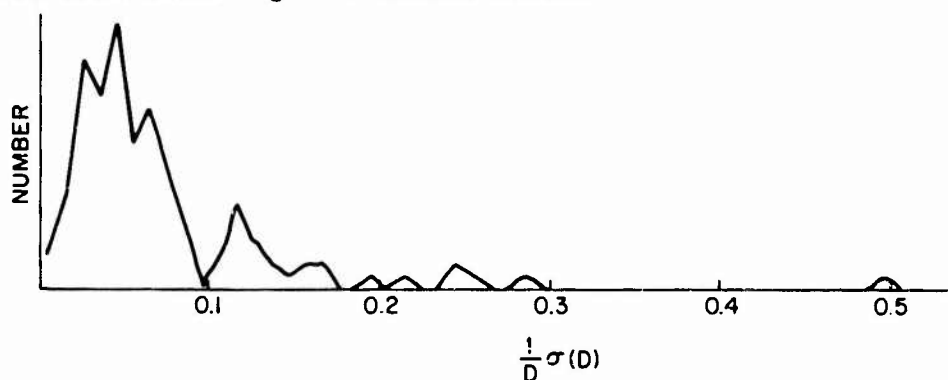

$$\frac{1}{D}\sigma(D)$$

Figure 3.

DR. PETERSON. Of the decay rate, you mean?

DR. SOUTHWORTH. One or the other. I am not solving any physics here, so far. Then I have taken the differences in height and decay rate and computed the derivative versus height in the atmosphere.

The next slide (Figure 4) shows a sample of the results I have gotten. A reasonable scale height would be all points on here at a scale height of about six, therefore inverse a scale height of 0.16. The mean fits. The individual meteors just don't. I have plotted it here as height, I don't know it proves a great deal, but there is no particular trend shown, and I didn't expect one.

DR. PETERSON. The vertical error was what on this?

DR. SOUTHWORTH. That is 6 km. I can move any one of these points in here, move it up and down.

DR. PETERSON. Yes, I was just wondering if that makes any difference. It doesn't seem to.

DR. SOUTHWORTH. Well, the next two things I will show you are really just representations of the same thing. The next slide (Figure 5) shows roughly that we are getting the correct height in addition to fitting a scale height. I fit a mean diffusion height for each meteor, and I have compared it here with a rather small sample of the height coming from the gradient in this geometry, and it seems to have a mean of about zero, with a standard deviation of six or more.

Now, I can look, however, at the internal probable error in finding the height from diffusion. You see, I have taken each point on this trail and using a scale height of six and a half kilometers have reduced all of these diffusions to the various stations into one height. This is the mean height for the meteor, and then from that diffusion rate, I have compared that with the standard atmosphere to find out what the diffusion height is. This is done again by the least squares and I get an internal probable error. The next slide (Figure 6) shows the distribution of these probable errors.

Since this is a probable error of the mean height, and I have normally three or four stations contributing, the probable error of individual diffusion heights is roughly twice this, so I would have four there. This is rescaled by about a factor of two. This is the error in diffusion heights from individual diffusion measurements. Since it has worked out in the differences in height, it doesn't matter that my other geometry is weak; it is just simply what we get.

Now, all the rest I have to say, and I guess I have already said it, is that the deviations I expect in the ionization curve from the moving point would give this order of difference. We propose to correct for it in the future, and we want to find out whether it would fit the dip. I certainly can't tell you now.

DR. ELFORD. What is the scale height equivalent to that least squares line on your optical illusion?
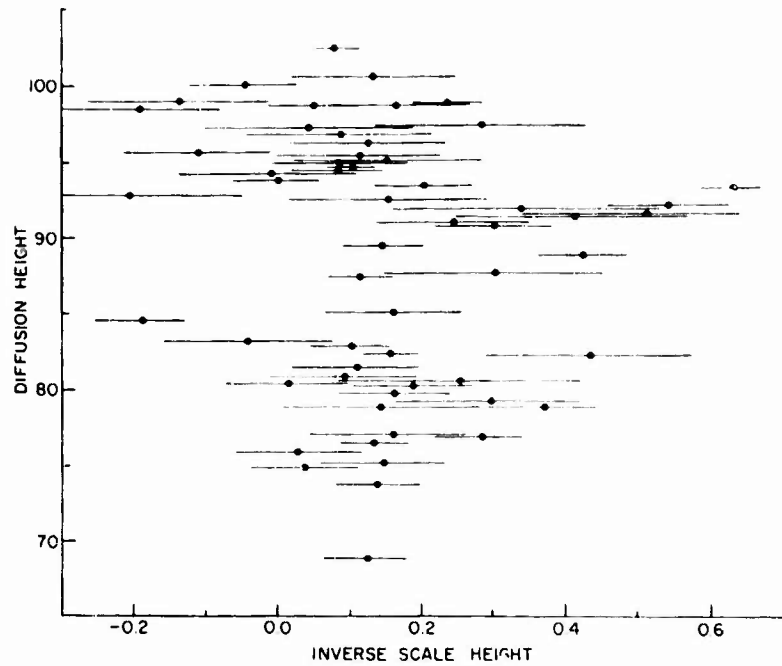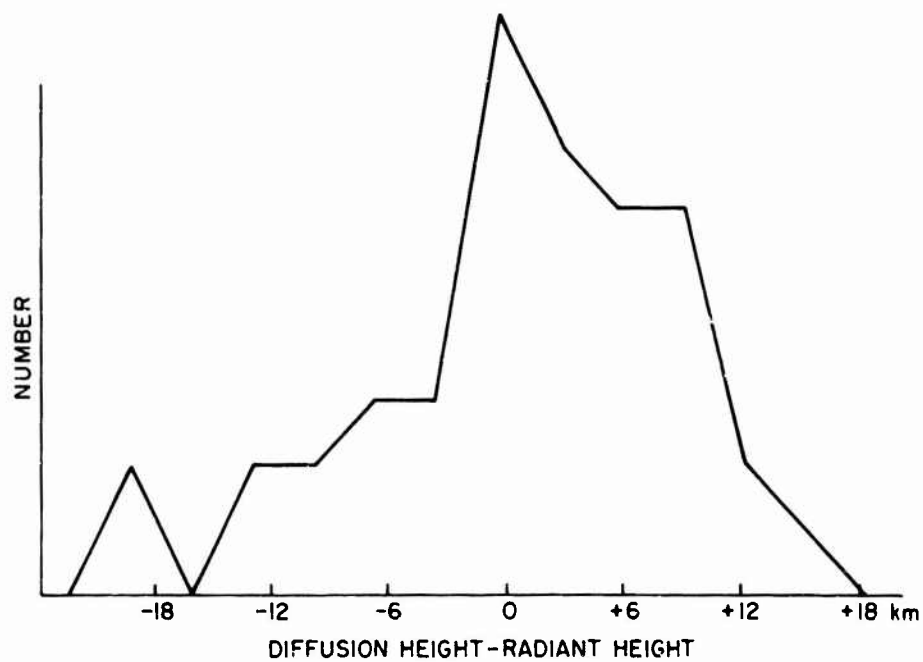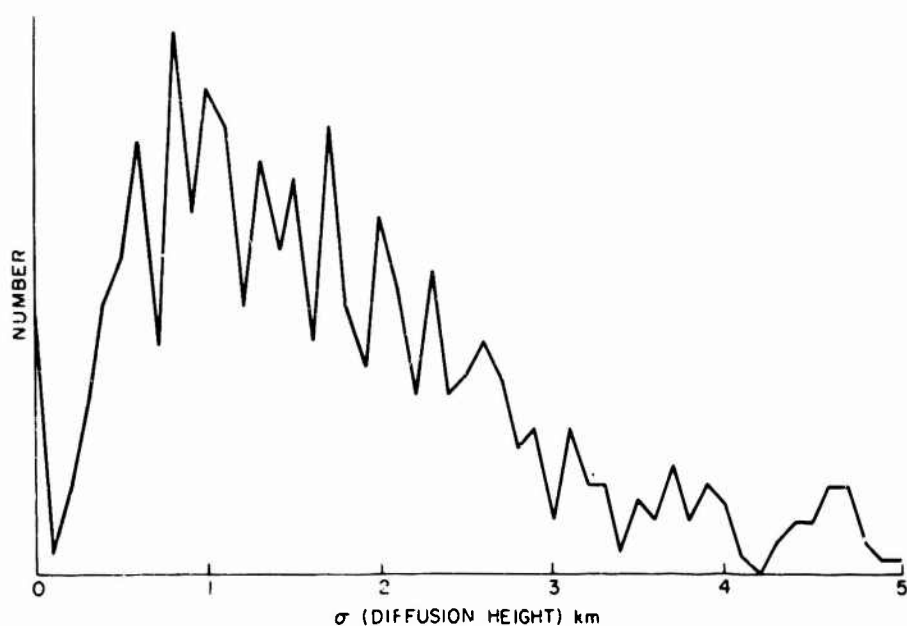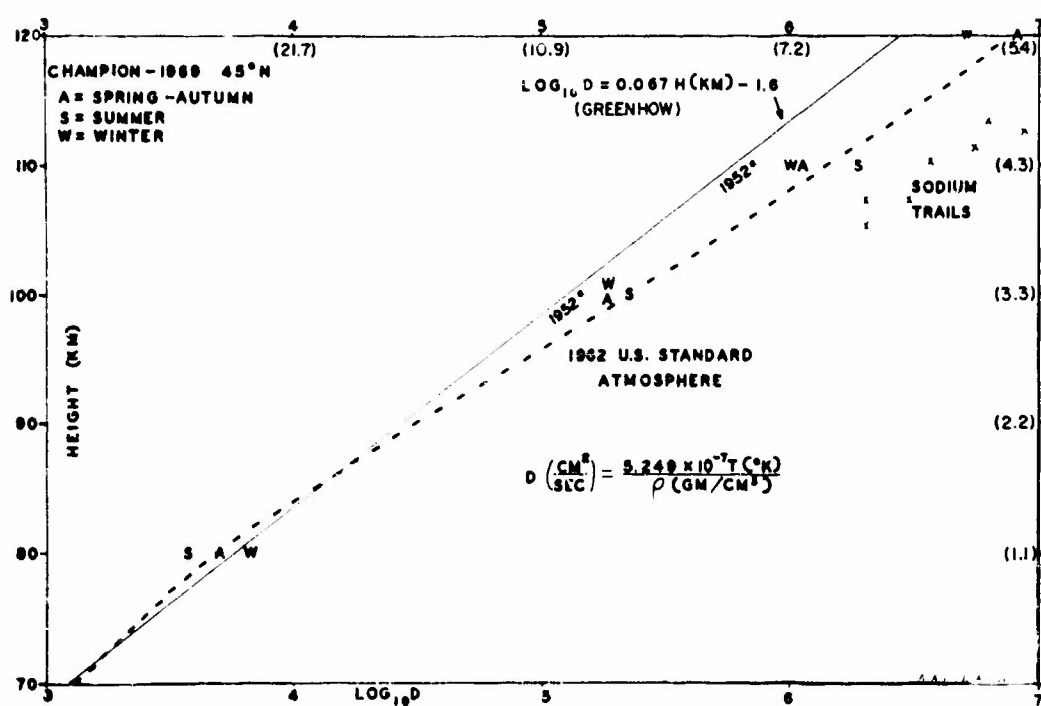
Figure 4.

Figure 5.

Figure 6.

Figure 7. Diffusion as a Function of Height

DR. SOUTHWORTH. I have forgotten. It has been too long. I have forgotten what it was.

DR. ELFORD. Did you say it was too low ?

DR. SOUTHWORTH. Yes.

DR. ELFORD. Somebody asked me what it was, and l said 20 km.

DR. PETERSON. Scale height ?

DR. ELFORD. Yes.

DR. SOUTHWORTH. Well, this isn't that bad. This was 9 km.

DR. BARNES. That is pretty large. If you look at the scale height computed from the U. S. Standard Atmosphere, 1962, for this region, the minimum value is about 5.4 km at 80 km and reaches 6.5 km at 70 and 100 km.

DR. ELFORD. I am not saying that this is the scale height.

DR. BARNES. This is another big question, is this slope really a measure of the atmospheric pressure scale height ?

DR. PETERSON. It is an indication of something.

DR. ELFORD. Dick pointed out this problem of the instrumental effect if you don't take into account that you may be losing diffusion coefficients to the top end. We had an argument with Greenhow. You find this in literature. We in fact did divide our scatter diagram into several parts, because we in fact ran on the recording system for a long time. We recorded very well the low end of the diagram. We divided that scatter block into halves; we analyzed the lower part, divided into thirds, and then analyzed the middle. We found that the analysis of the whole diagram and the middle and the lower half all give the same scale height.

DR. BARNES. One of my slides (Figure 7) shows scale height. The slope of the solid line, given by Greenhow's equation, gives a scale height of 6.5 km. The numbers in parentheses along the top and right side are the scale heights for straight lines drawn from the tick marks to the lower left hand point of the diagram.

DR. PETERSON. Graham's is 20.

DR. BARNES. That would be a very steep line on my diagram.

DR. ELFORD. If you go through Greenhow's theory, it comes out to be the same.

THE FLOOR. Yes, it comes out to be steeper.

DR. PETERSON. What did yours come out to ?

DR. SOUTHWORTH. I don't know; l haven't done it. (Ed. note, the line on Figure 1 gives an 8.75 km scale height.)

DR. ELFORD. It is interesting that Dick's main scattering might work out on his differential. It is about right. This is interesting to me.

DR. BARNES. What was the value that you got on the differential (Figure 4) ?

DR. SOUTHWORTH. Something like six for the scale height.

DR. BARNES. That is not bad over the whole range.

MR. MÜLLER. What height range are you thinking of, —— the whole meteor range?

DR. BARNES. No, if you look at the slope of the curve, it changes, and it was in the 80 km region that I got the 5.4.

DR. ELFORD. Maybe I could ask Dick again to run through the physical argument why you would expect to get this large variation in the diffusion coefficient along the trail. This seems to be the crux of the matter.

DR. SOUTHWORTH. McKinley says that the amplitude will vary due to the diffusion as exp $(-16\pi DT/\lambda^2)$, and if I put in my wavelength, that becomes exp $(-2.84 DT)$. D here is in square meters per second and is of the order of three at 90 km. That is just what happened because of the diffusion. Now, if I plot the log q as a function of position X on the trail -- I had better plot something like this, because that is what I am doing, I have a curve like this, and as I said earlier if this is down by one magnitude, this length is of the order only of 6 km. (Diagram not available.)

DR. PETERSON. Doesn't the q terminate on the bottom sharper than that?

DR. SOUTHWORTH. Out here I haven't observed anything, so it goes down photographically like this; I have seen them go down. I suppose that the electrons do something comparable, although this is shorter than most meteor trails. Then, of course, there were height variations. Now, I started out with the reflection point here, and because of wind shear, it moved this way. So, my amplitude would also vary, not only due to the diffusion, but due to my shifting to a different number of electrons to begin with; so, I get something of the form, transforming from magnitudes properly, exp $(-.921$ times the magnitude$)$ or sometimes you are concerned with the derivative here dM/dt, and I convert dM/dt into dM/dXo, where X_o is the position of the reflection point. dXo/dt from the algebra that I had yesterday is $-\alpha/(1-\alpha V)$, and alpha, as I said, is plus or minus 0.1. Extreme cases will go far enough so that the denominator is a nuisance, but normally not. The point is that this reflection point is moving on the order of 10 percent of the speed of the meteor. This slope works out to be of the order of 0.3. Putting in 0.3 and, say, velocity of 30, I come out with the variation exp $(-10\alpha T)$. Alpha, as I said, normally will go to 0.1, to give exp $(-T)$. If D were 3, that is 10 percent of this change. If D is smaller than that, it is more important.

DR. PETERSON. Let's see, when you say that alpha is 0.1, that slope 0.3, how do you decide that that should be 0.3?

DR. SOUTHWORTH. Well, actually, in 3 km I came down by one magnitude.

DR. PETERSON. Yes, but how do you know that you are on the peak to start with?

DR. SOUTHWORTH. Well, actually, the best I can draw these things looking at the Fresnel patterns -- it doesn't have a pattern like that, I have one that goes

like -- I don't know like what. I said that I expect more irregularity than I have here. However, I am not going to do very much with these yet, because I have a great deal of difficulty calibrating between my stations, so I can't very well compare answers from one station to another.

DR. PETERSON. You don't know that you are on the peak, but you think it varies more than what you are saying?

DR. SOUTHWORTH. Yes.

DR. PETERSON. I see.

DR. ELFORD. With this sort of reflection point motion, what would be the change in the Doppler that one observed if you are measuring wind? Would it be sensitive enough to detect it?

DR. SOUTHWORTH. It depends upon how long you observe this Doppler.

DR. ELFORD. All right, yes.

DR. SOUTHWORTH. I thought that with the accuracies I expected to have that, in something like a tenth of a second, if the shear was important, I would probably detect it. However, I note that I get one value of the phase at every pulse. I don't really have zero crossings.

DR. ELFORD. You can have the shear by looking at the two individual reflection points and checking these.

DR. SOUTHWORTH. Yes. One of the things I really hope to find out is whether it is fair to go from one reflection point to another.

DR. ELFORD. That would depend upon your separation.

DR. SOUTHWORTH. Well, this is 3 or 4 km.

MR. ZIMMERMAN. Some of the small scales we observed, smoke trails and chemical releases, have lengths to the order of as low as 3 km, especially at the altitude you talk about there.

DR. SOUTHWORTH. Well, I thought I observed a few lovely kinks in the optical trail.

MR. ZIMMERMAN. Yes, very, very lovely.

DR. MILLMAN. You say that you got 0.921?

DR. SOUTHWORTH. Well, that is essentially transforming the two systems of measuring magnitude, decimal and exponential. It is 0.4 times the natural log of ten.

DR. BARNES. Any more questions?

DR. ROPER. I have a feeling that one will be able to approach this from the following point of view. If you had such a large wiggle in your wind velocity, this is certainly going to affect your Fresnel pattern formation. So, there will be times that you will be able to look at your records and justify almost completely an interpolation. There will be other times when the Fresnel pattern will show distortion, particularly if it involves something like this in terms of wind relations between

the reflection points. Under these circumstances it might be unwise to try inter-
polating, but it will be interesting to see how your answer comes out.

DR. SOUTHWORTH. We are simply trying to collect all the data we can on
the individual meteor.

MR. MÜLLER. Since we are all interested in observing the decay for as long
a period as possible, it is really advisable to work at all frequencies for these
studies.

DR. BARNES. Yes, and 1 am going to ask Allen to discuss this after the cof-
fee break. It is something that I think we should investigate at this time.

I would like Professor Ellyett, from the University of Newcastle, Australia,
to tell us what they have been doing and what they plan to do.

III. The Work at Christchurch, New Zealand

Prof. C. D. Ellyett
University of Newcastle
New South Wales, Australia

The first few moments will be concerned with the history of our group in the southern hemisphere, followed by some actual meteor results.

I realize I must be getting old, because my interest in the subject extends from the days when the work at Jodrell Bank was carried out in an onion shed and I had the pleasure of being the first one to see a Fresnel diffraction pattern. I realize now the amount of trouble that has subsequently caused. However, it was after that three years at Jodrell Bank that I then went out to the University of Canterbury at Christchurch, New Zealand, to initiate radar observations of meteor showers there. That work was carried out for some 15 years operating on 69 MHz and we had ideas, after we obtained the southern hemisphere showers, of obtaining velocities and hence complete orbits. As I mentioned this morning, we ran into so much trouble and strife trying to get accurate velocities that we decided to switch ultimately from measuring orbits to further work on meteor rates.

The first period of some four or five years there was carried out measuring showers only, and at this particular point I would like to say what a great advantage and help it was to us to have financial support from the AFCRL, because without that the work would not have been carried out at anything like the same rate. Then, when we went over to measuring meteor rates, we had support from NASA, and that support has continued up to the present day.

I would like to mention three people's names, because these crop up later on: viz, Dr. R. G. T. Bennett, Dr. G. J. Fraser, and Dr. C. S. L. Keay. Now, these three people were graduate students working in the field of meteors there. Dr. Keay subsequently joined the teaching staff at Canterbury and continued in meteor work. In 1960-1961, we decided to carry out a survey of rates with absolutely constant equipment parameters. This meant that the parameters were automatically checked internally, and at the end of the year we could say that the rates we were obtaining were strictly comparable with the rates at the beginning of the year.

Subsequently, the idea came forward from Australia that there was a lunar influence on the rates, and to check that, we commenced another survey in 1963, again with the same equipment, and again, with exactly the same constant parameters. This is one of the key things in this sort of work.

Then, at the end of 1964, I transferred to Newcastle, a hundred miles north of Sydney, in Australia. The survey in Christchurch continued until August, 1965, so there were some two and a half years of the second continuous survey there. At that time Dr. Keay transferred to Newcastle with me as did the whole of my research team, and, of course, that is a very big help if you are starting work somewhere else.

We have not completed all the analysis of the second survey. The first year or so of it we completed in Christchurch to establish quite clearly that there was no lunar influence on the meteor rates. Of course, that has now been published. A new University campus, a new Department and a new field station have been constructed at Newcastle in the past two years and we are now getting back into some real work again. The first thing that we are doing and have almost completed is the completion of the analysis of the two and a half year's survey.

The proposal now--it certainly was only an idea before I came here this week-- is to construct some new equipment. What we propose to do is to build equipment with constant parameters again, to drop down in frequency from some 69 MHz to the region of 40 to 45 MHz, to increase the pulse recurrence frequency, to recover echoes automatically out of the noise by an integration effect, to have reasonable power, and to go to automatic recording. We have in the past employed girls for film reading, but it is ever so much easier these days to automate for this sort of work. Newcastle has acquired an IBM 1130 computer, which is quite a good one, and having that available, we should be able to be operational in this field in the not too distant future.

The idea is to make two identical sets of equipment, to put one in the northern hemisphere, and to let the two run for a number of years measuring rates and seeing what variations there are in rates from year to year. I hope this will come to pass. I have heard this week that money will probably be forthcoming for this project. What has happened to Christchurch is quite clear. No winds have been

measured there up to the present time, but the first two people mentioned previously have subsequently returned from England and have joined the University of Canterbury staff. They have obtained an NSF grant, and are going to measure winds at Christchurch. That is now being instrumented. I don't know how long it will take, or any of the details of precisely how they are going to go about it. But it will mean that in maybe six months or a year's time there is every chance that there will be at least two places in the southern hemisphere that will be obtaining winds with meteor techniques.

The other interesting point is that Dr. G. Fraser is not working on meteors. He has been working for a year or two now on winds by ionospheric methods, and the two methods will be working at the same place. That also could be of interest. Work will be going forward at Christchurch, and our intention at Newcastle is to concentrate on meteor rates.

I would just mention that if anyone had any apparatus that was available, it could be quite interesting to have three sites in the southern hemisphere measuring winds, because otherwise the southern hemisphere is completely untouched. Adelaide, of course, as you know, has been doing this for a number of years.

Now, one point that could be of interest to this meeting is that the number of meteors your equipment records, if you are measuring winds or anything like that, is apt to vary from year to year.

Figure 1 gives the rate survey from the 1960-1961 data obtained at Christchurch. In 1963 we found that our rates were creeping much higher than they had been for the earlier survey. We did some sort of agonized reappraisal of our equipment in every respect to make sure that it hadn't changed. We couldn't see any cause for it changing, so we wrote to Dr. Millman at Ottawa, which I think is the only other place where they are doing this sort of systematic work. We didn't tell them what we had found. We just asked if they had anything of interest happen in 1963, and they replied that, from about I think it was April through to about September, their rates seemed to have gone up by about 50 percent. We were very happy about that, because then we knew it wasn't equipment. We knew that world-wide, for some unknown reason, the rates had practically doubled. There was a 50 percent increase in the northern hemisphere, and over the same time, a 100 percent increase in the southern hemisphere. It dropped down pretty well to normal by September 1963. In the beginning of 1964, there was a slight increase, but further work that we carried out at that time had shown that 1964 rates almost returned to normal.

Figure 2 gives contours of activity for the first 17 months of 1963 and 1964. The highest rate is the Delta Aquarids, which is the strongest meteor shower that impinges on the earth. It is not so evident at high declinations in the northern hemisphere, but it is a very strong shower. One point which is of interest is that when
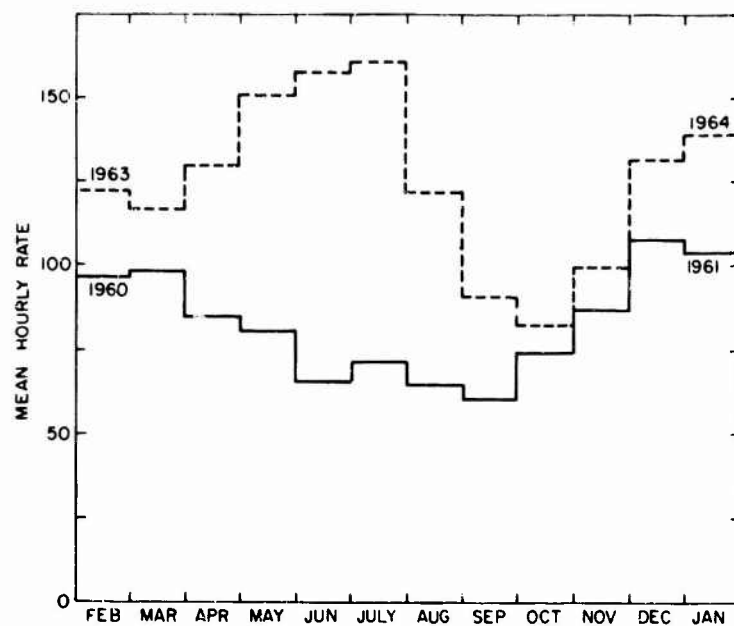
Figure 1. Mean Hourly Rates of Meteor Echoes Observed
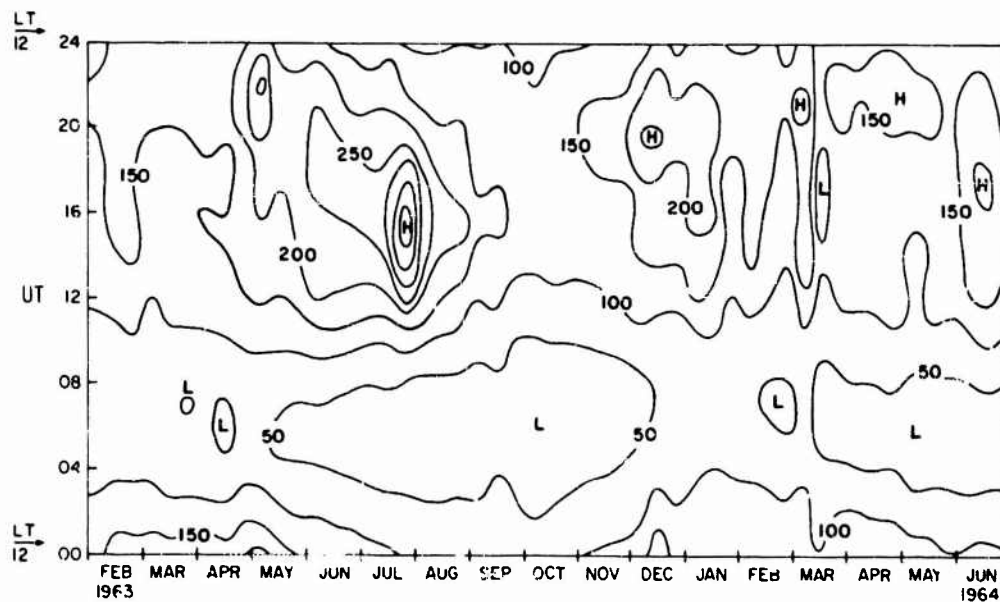by Radar From Christchurch, New Zealand

Figure 2. Mean Hourly Rates of Meteor Echoes Observed by Radar
From Christchurch, New Zealand

you go down to smaller meteors you find that what might be a visual shower over a day becomes a radar shower over weeks and gradually builds up to the visual peak and then dies down again.

The 1960-1961 survey gives the same sort of contour picture in general and reinforces the general structure of occurrence of meteors in that way.

One question that arose was: when the general rate goes up, do the shower rates go up as well? If we take 1960, the delta aquarid rate is about a hundred per hour. At the peak of the Delta Aquarids at 3:00 a.m. in the morning, averaged over several days, it goes up to 210 per hour. That is an increase of something like 110. In 1963, where we had the high rates, it was 300 around the Delta Aquarids and went up to 454, or an increase of 154.

It is a little uncertain what one can see from that, but one can draw a reasonable conclusion that the shower itself is not increasing in rate; it is merely all rising up together with the shower sitting on the top of the general rise in the activity level. We have been continuing this analysis. If we just take 1960 again during the Delta Aquarids, but look at 5:00 p.m. where there are no Delta Aquarids in the sky, we should have the smallest rate of the day. Now, in 1960, there was a rate of 210, as we saw at the peak of Delta Aquarids, falling down to 15 per hour at 5:00 p.m. In 1963 it was up to 454 at 3:00 a.m. and 30 at 5:00 p.m., and in 1964 it was down again to 244, almost down to the 1960 figure, and the 5:00 p.m. figure was down to 20. So 1963 was an abnormal year when the meteor rates went up.

Last week we finished getting out the 1965 results. It was 425 and 35. In other words, the rates have gone way up again. In two alternate years now, we have this indication of a very big rise in rates.

DR. PETERSON. You say that is a very big rise in certain sizes?

DR. ELLYETT. This is down to a magnitude of plus eight. All sizes are integrated down to that.

Now, there are some very odd things happening here. In 1963, and again in 1965, it is very hard to say how interplanetary matter could have increased over a half a year coming onto the two halves of the earth. It seems really hard to credit that that could happen. It seems odd if you try to attribute a natural ionospheric explanation to it, because in Canada where the frequency used was lower, the rate increase was also lower, and that would be the wrong way to what would be expected from any general ionospheric cause. There is one other alternative, a man-made cause, namely the possibility of perhaps a small amount of material getting put into the extreme upper atmosphere, and having some sort of catalyst or triggering effect. At this stage I am not prepared to rule that out, but you can see that we have some quite interesting unanswered questions here, and this is why we want to continue with this work as our first objective that we are going to attack and why we want to do it in the two hemispheres.

Well, I thought that you would just perhaps be interested in these few remarks showing that the work at Canterbury is continuing, and it is going to move into winds while we at Newcastle will be studying meteor rates.

DR. BARNES. Thank you, Professor Ellyett.

DR. ROPER. Dr. Ellyett, could you give us some idea of the beam width?

DR. ELLYETT. Omnidirectional.

DR. ROPER. It is omnidirectional?

DR. ELLYETT. It is all done with an omnidirectional aerial.

MR. MÜLLER. Is there any information available for years before 1960, say for a period of two years, to compare rates as they change from year to year?

DR. ELLYETT. I don't think so.

DR. PETERSON. We have several years that we could drag out on 40 MHz.

DR. ELLYETT. Did you have constant equipment parameters?

DR. PETERSON. With rotating antennas, but they were doing the same thing that you were. I don't think I have that with me, but Eshleman ran this for three years before 1960.

MR. MÜLLER. I am thinking of the old Jodrell Bank equipment, which used to go round the clock. I have never seen anything on this.

DR. ELLYETT. Knowing that equipment, I wouldn't trust it, because the sort of things that we were doing, including an automatic noise check every three minutes, and automatic transmitter output recorded every hour have to be done before you can rely on your equipment over long periods.

DR. MILLMAN. Our rate measurements extend over eight years, but we had five years data at this period of the year. The deviation was about 20 counts per hour, averaged over the five years, and the rate at this period increased about 100 counts per hour in 1963. In the five years previous to that our deviation was only 20 counts per hour.

DR. PETERSON. You have the answer, obviously then.

DR. MILLMAN. We do have a problem though, and we will have a better estimate when we analyze our records after 1962, because the first two years of our records were seriously hampered by the high sun spot activity, and there were far more days when we didn't get complete 24-hour records.

THE FLOOR. With the standard deviation?

DR. MILLMAN. Even with that, but the analysis of the 1963, 1964, 1965 years will give us a much better estimate because we were not bothered by the sun spots.

MR. MYERS. Will the Christchurch people make any attempt to measure height along with the wind data?

DR. ELLYETT. I can't tell you specifically what they are going to do because I am away from there now.

MR. MYERS. I just thought that maybe you could read their minds.

DR. ELLYETT. What I imagine they will do will be to add cw to the straight radar pulse technique. I haven't heard exactly what they are going to do.

DR. BARNES. On Dr. Fraser's work with the ionspheric drift, does this give heights any better than they had been in the past?

DR. ELLYETT. I think it does, but I can't speak with authority on what he is doing. It has been two years since I was there.

DR. BARNES. Do you know if this has any hopes of giving better height than in the past?

DR. PETERSON. I don't know of any way to do it, but maybe it does. All of our attempts to look for these variations in signal and correlated different places left us not knowing where the height is.

DR. BARNES. This is our reason for using meteors.

DR. PETERSON. This doesn't prove that it won't work. I just don't know how to do it.

DR. ELLYETT. There is one thing that I noticed very much, even shifting to Australia from New Zealand, and that is that New Zealand is ideal from the point of being a low noise site. It has got it over everywhere.

DR. PETERSON. New Zealand?

DR. ELLYETT. Yes.

IV. Density Versus Height*

Dr. Issac Revah
CNET, France

DR. REVAH. We have put here on this graph (Figure 1) the height by echo decay method based on Greenhow's slope and the height we measured by our radar. It was done on almost 100 echoes from 5 o'clock in the morning to 12 o'clock, and we have selected only echoes near the axis of our beam. That means we have chosen the resultant coordinate toward the south and the north and limited the height interval to about 20 km.

We have three lines; the center one, corresponds to the ideal line which we were supposed to find, and which gives the same height from the radar measurement and from the echo decay method. The other two lines are supposed to be ± 2.5 km apart from this line, and what I called yesterday strong correlation was the fact that we found that most of the echoes are within this height interval of ± 2.5 km. We naturally have sporadic echoes far from these conditions, but now I am not even really sure what they are measuring. We have not yet made any computations on density variations because of the small number of echoes we have.

MR. MYERS. You felt good about it until today.

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*(Dr. Revah consolidated his remarks into one extended abstract which has been placed with the other papers presented on the first day of the Workshop, page 83.

Since some of the material covered by Dr. Revah does not appear in detail in the abstract, the editor has taken the liberty to include some parts of Dr. Revah's talks as they were recorded by the stenographer.)

DR. REVAH. We felt that other people were able to obtain records in agreement with the echo decay method. We knew there was a problem on the diffusion coefficient and the variation as a function of time or the function of the altitude and seasonal variations, but those things have been written quite a few years ago. We thought at the present time we were more or less sure of these things. We were aware of the interference of the ionization which is not uniform on the trail. And in all our measurements we supposed that the irregularities of the ionization on the trail were of a dimension comparable to the first Fresnel zone, so we were not expecting to have trouble with the first few Fresnel zones on the trail. In fact, now I am not very sure what l am finding. I am going to look at this very much closer. We have a strong correlation on Figure 1. It is not haphazard. It was not a thing which we weren't waiting for. We looked for good meteors and good echoes on which we were able to look at the decay of the amplitude. That means that we needed at least 14 dB decay between the maxima and the minima of the echo.

We selected echoes which didn't present any shift of the Doppler phase, and we used only the north-south direction because we were afraid of secondary loss on our antennas. We were not very happy on this first result. We thought it was more or less common. Now l am looking at it with new eyes.

DR. BARNES. I see that you apparently had one of the difficulties that I did in reading off the density. We were taking the density slope, and I was having girls read this and they would read it to the nearest five degrees or ten degrees and l got things like this, too, where they all seemed to line up in certain places. I had to have them reanalyze the data.

DR. REVAH. We had curves which correspond to the height intervals from the decay method. We did not compute these things on the computer.

DR. BARNES. I find that by having the girls do it by hand they are influenced towards certain values whereas if the computer does it, there is no such influence.

DR. REVAH. That is right.

DR. SOUTHWORTH. You say you have eliminated those with a change in the Doppler frequency, therefore, you have essentially removed the difficulty of moving reflection points.

DR. REVAH. Yes, l suppose so.

DR. SOUTHWORTH. Well, I had said that I thought that a large part of the difficulty that l was getting on my data was waiting to see what was left over. If I find this much residual, I was looking for a new club. But I don't see that you have reason to worry about that; your data are too good.

DR. PETERSON. Well, this is fitting Greenhow's straight line, not his data.

DR. ELFORD. I was trying to interpret this. This is a comparison between your heights as measured directly with those inferred from using Greenhow's curve, which we now know is wrong, so, this is going to make it a double
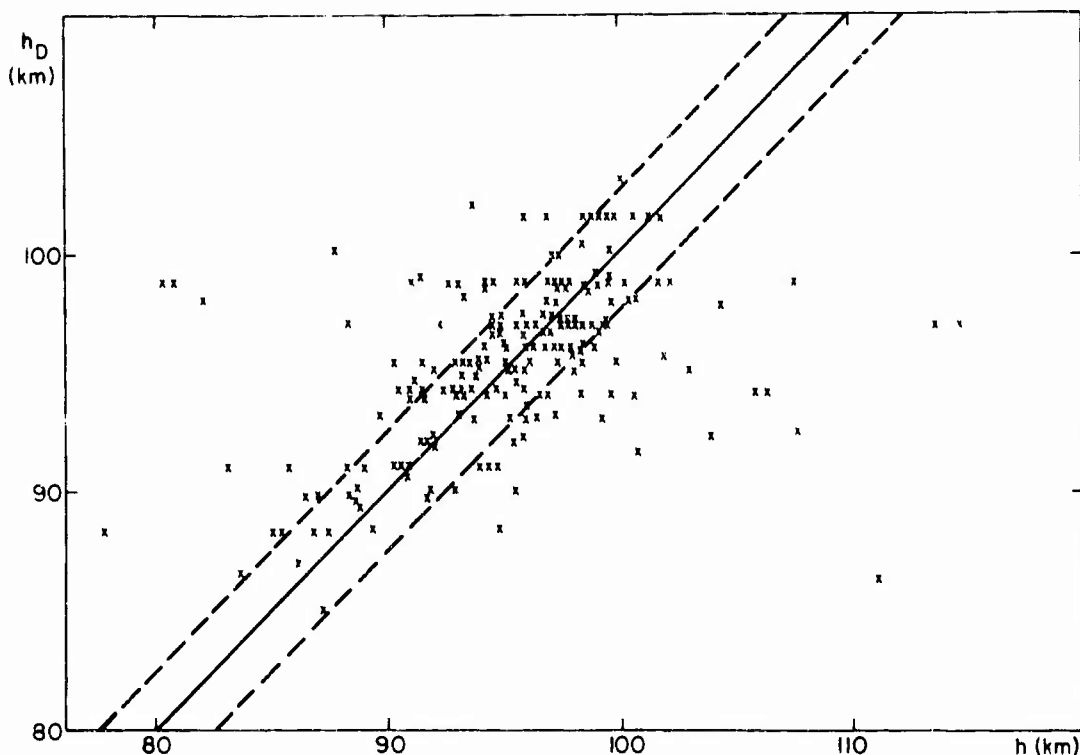
Figure 1. Comparison Between the Height h_D Deduced from the Exponential Decay of Underdense Echoes, and the Height h Measured by the Garchy Radar (December 12 and 13, 1965)

interpretation. We also believe that Greenhow's curve is what you would expect without scale height, so it is probably that you are getting the right scale height.

MR. MYERS. Isn't it a little misleading to say that Greenhow's data are wrong? It does fit the data; it doesn't fit the theory.

DR. ELFORD. This is a method of scatter diagrams.

DR. PETERSON. His straight line may be right.

DR. REVAH. We hope so, we used it.

DR. ELFORD. What he did was quite correct in his final work. He used a straight line to read back his heights. That is all right; you can go in that direction, but you can't go in the opposite direction.

MR. MÜLLER. That is what he intended.

DR. ELFORD. That is right.

MR. MÜLLER. We have used the same method.

DR. ELFORD. Yes, there is nothing wrong with it.

We looked at some echoes way back in 1952. We were delighted to find that when we plotted the diffusion coefficient of these echoes against height, we got relatively good scatter, and we got the right scale height. We were greatly

encouraged by this. These were the first decays we ever measured. Since then, it got worse and worse until the next general shower. I wonder if anybody else had the same experience, it is an unusual thing.

DR. BARNES. This is the Geminids in December?

DR. ELFORD. Yes.

DR. BARNES. Are they dust balls?

DR. SOUTHWORTH. No, they are not. They are remarkable for their smooth, long, light curves.

DR. ELFORD. This might be a clue for this problem.

DR. BARNES. What is their average height?

DR. MILLMAN. They are lower than most.

DR. SOUTHWORTH. But they are remarkable for being strong solid objects.

DR. ELFORD. We might suggest it has something to do with irregularities of ionization along the trail. These have a greater effect than just the shearing effect.

DR. MILLMAN. Again, they are unique in being the strongest shower we encounter as far as our northern hemisphere showers go. I am not too sure about the Delta Aquarids, because we can't compare the record, but the feature is that they have both the large and the small meteors in them. They peak well whether you plot for small meteors or large meteors, which is quite different from all the other meteor showers observed in the northern hemisphere.

MR. NOWAK. Am I right to interpret your remarks concerning this that the decay-height is not simply the occurrence of a shower but of this specific Geminid shower?

DR. ELFORD. Yes, we have looked at all other showers and they generally look like the sporadic meteors in regard to this scattering in the decay.

V. Additional Thoughts

Prof. Allen Peterson
Stanford University
Stanford, California

Since talking with various members of the group here, I think what I have to say is certainly not unique. Everyone suggests that it would be useful to operate at lower frequencies in order to obtain data on meteor trails at greater heights. As the height of the trail increases, the initial radius of the ionization and its diffusion rate increase. For a given frequency the initial echo strength and its duration decrease as the height of trail increases.

MR. MÜLLER. I think Greenhow's final views were that the initial radius was sensitive to a number of things.

DR. PETERSON. Yes, that is a different question that we ought to explore while here. But, in any event, a cutoff height exists near 105 km for frequencies above about 30 MHz. I think that the only way that we can study trails at greater heights is by going down in frequency. Lower frequencies present obvious difficulties, not the least of which is the frequency assignment that someone called to our attention.

One of Arnold Barnes' equations showed that the received echo power increased as λ^3 with an antenna of the same gain for transmit and receive. This looks good but it is found that the noise power also increases at about the same rate. When cosmic noise dominates it varies as $\lambda^{2.3}$.

MR. MÜLLER. 2.5, I think.

DR. PETERSON. No, I believe it is $\lambda^{2.3}$, but that isn't the key thing. In the frequency range below about 30 MHz ionospherically propagated atmospheric

noise (from lightning) dominates cosmic noise most of the time. However, during the daytime D-region absorption must also be considered. I conclude that the noise level generally tends to increase from 30 MHz down towards 10 MHz at about the same rate as the signal increases. It could be much more unfavorable than this at a location with nearby thunderstorm activity.

The Doppler frequency varies as λ^{-1}, the duration as λ^2, and hence the number of cycles of Doppler frequency during the echo varies as λ. Thus if the signal-to-noise ratio is approximately constant, measurements at lower frequencies should certainly be possible. However, interference is often worse as the frequency is decreased. Another real problem, which I see becoming important as the frequency is lowered, is back-scattering from distant regions on the earth's surface. Because of the high repetition rates used for the meteor Doppler observations, the large time delay ground back-scatter echoes are observed simultaneously with the meteor echoes. In the daytime, these echoes may be the most difficult obstacle in the use of lower frequency observations. At night, less difficulty from ground scatter will occur.

One of the things that we have been studying which may alleviate the effects of ground scatter is the use of phase-coded transmissions. Phase-coding can be made to be responsive to the meteor range of time delay and to discriminate against the longer delays associated with ground scatter. A reduction of the unwanted signal of the order of 30 dB can be obtained in practice by the phase coding technique.

Another feature of ground scatter is that the echoes are typically Doppler shifted less than a cycle per second. The reason for even this Doppler shift is that the F-layer effectively moves up and down during the day. One can visualize all echoes of this kind, and even all those reflected at vertical incidence from the ionosphere as contributing signal energy in the Doppler frequency range near zero shift.

On the other hand, the most interesting meteor echoes have Doppler shifts extending to about 30 Hz at 30 MHz and to 10 Hz at 10 MHz. So there is a possibility of separating ground scatter from meteor echoes with narrow band filters to reject all spectral components near the carrier frequency.

MR. MÜLLER. But it will come from sporadic E which falls more rapidly.

DR. PETERSON. Perhaps, it can be, but most of the time it isn't, based on our measurements.

MR. MÜLLER. Well, I have seen some that looked as if they did.

DR. PETERSON. Well, there may be some sporadic-E echoes with large Doppler shifts and all auroral echoes are likely to be troublesome. However, auroral echoes can be rejected by not directing the antennas towards the north (in the northern hemisphere). Much of the time (from our observations most of the time) a good notch filter could be made to work well. It is somewhat less clear

to me how much we can extend the height of observations by going down in frequency because if the initial radius increases too rapidly with height, then there is nothing that we can do. We once looked for meteor echoes on a frequency near 5 MHz, and were disappointed by how few were observed. I don't know the reason for this but it may call for systematic investigation. I would like to at least try again since calculations seem to indicate reasonable numbers should be observed. While here I have heard that others of you are interested in going to low frequency, and in fact that the group in Adelaide was thinking of frequency as low as 3 MHz.

DR. ELLYETT. That is half constructed now.

DR. SOUTHWORTH. You wondered how high you would go?

DR. PETERSON. Yes.

DR. SOUTHWORTH. Do you have some other theory on the dependence on initial param   r and diffusion on anything other than the vertical variation of the density of the atmosphere?

DR. PETERSON. No, not really.

DR. SOUTHWORTH. If this is a simple-minded theory, it defines exactly how far you will go from wherever you were.

DR. PETERSON. That is true. That is right. There is still in the literature some uncertainty as to what the initial radius is that diffusion starts from, and, therefore, there is some uncertainty as to the number that is calculated.

MR. MÜLLER. I think this actually would still be more relaxed at the lower frequency.

DR. PETERSON. It is bound to be better at the lower frequency.

MR. MÜLLER. How can you observe anything at 10 MHz? The problem is interference, obviously, because the band is practically dense, so you have to pick a channel, and you are going to have to take power from another frequency. It is not always very pleasant to do that.

DR. PETERSON. I don't disagree with that, although I think that is what would have to be done for short periods of time. We have successfully run our backscatter at 12 MHz for a number of years. Interference has certainly gotten worse.

DR. BARNES. What about the accuracy of the elevation angle, doesn't this go down?

DR. PETERSON. If we are successful with our simple scheme of measuring at these relatively high angles, it will not then be too difficult to build a similar sort of comparison array at 10 MHz. I would not like to try to build highly directive ones purely for this experiment; on the other hand, the Adelaide group is building some very big ones, a kilometer square for use on 3 MHz.

DR. ROPER. It will take to the Doppler turned down to 2 MHz. Each one is fed with individual feed lines. We can then take the diffraction we get out of this

and place the antenna wherever we want to. Graham told me it was 89. It was supposed to be 100 antennas, ten-by-ten array with the whole middle field in.

DR. PETERSON. That turns out then to give you 10 half wavelengths or 5 wavelengths?

DR. ROPER. I am not too sure.

DR. PETERSON. Even at that, with this monstrous size, the array does not give a very narrow beam. It is ten degrees or so.

DR. ROPER. However, of course, this is an adjunct to our present system. Therefore, we will know where they are, because of the other one.

DR. PETERSON. That is possible in this case because one is then sure that the reflection point stays put. There are a couple of other features here that I glossed over. One must go through a certain amount of E region in order to get to the meteor trail. When we go to lower frequencies, "Faraday" rotation can be enough under some circumstances to preclude receiving on the same antenna that is used for transmitting. In one quick estimate, that I am not sure that I should reproduce here, I found there were something like three cycles of Faraday rotation at 15 MHz during the day. The same integrated electron density would give 0.6 km range error in the measurement if you didn't correct for the group retardation.

On the other hand, at night even at 10 MHz there is virtually none of either one of these effects.

DR. BARNES. 0.6 km? Well, that is 10 MHz.

DR. PETERSON. Yes, it is. I calculated 640 meters, to be exact. Since these are sort of notebook scribblings and not subjected to the scrutiny of my colleagues, I shall not push the point. I just use it to call your attention to the fact that we should do the calculations carefully. Absorption in the D region when operating at lower frequencies may also be important in the daytime.

MR. MYERS. What would be the effect of using an 18, 36, and 72 MHz sort of a multi-frequency affair; it could make life so complicated that you would never get any data out of it?

DR. PETERSON. I don't think that it means that you are complicating yourself at all.

MR. MYERS. If you would do everything at 18 MHz, and then just use it on harmonics all the way down.

DR. PETERSON. Well, you could do that or you could decide which frequency it was that you wanted to do most of the reduction on and only use the other one for decay rates or Doppler or something of that kind. Presumably the only reason for using the lower frequency is to obtain data on meteors at greater heights. I have been particularly interested in this in that I would like to get it up to the height range in which the dynamo currents flow. It may not be possible, but aurora echoes

do occur from heights like 105 km as does sporadic E. Auroral echoes appear now to be largely controlled by the current systems that flow and generate plasma instabilities. It would be interesting to know whether or not the region is moving and causing the currents or whether they are being driven from the magnetosphere. I don't think I have any more on this at the moment. I hope to try it sometime.

DR. BARNES. Do we have any more comments?

MR. MÜLLER. I would like to go back to the original question of the sporadic E, because I remember one particular record where I saw it being produced where instead of a steady decay rate the signal varied on the order of 1 Hz; but on the Doppler display there were rapid changes of at least 20 Hz. It may be exceptionally high, but it still has to be borne in mind.

DR. PETERSON. I am sure of that, but let me ask through, at what range was the echo?

MR. MÜLLER. Possibly between 500 and 1,000 km.

DR. PETERSON. This is ground backscatter?

MR. MÜLLER. Yes.

DR. PETERSON. Well, I would propose to reduce it in amplitude by range gating. Maybe that will work. As I see it, one has to use both kinds of gating in order to get around the interference.

MR. MÜLLER. Yes.

DR. PETERSON. There is another trick that may be useful. It is possible to change frequencies from pulse to pulse by small amounts. If you do this right, you can still extract the Doppler and certainly can still extract the amplitude. Then, you don't come back to the original frequency until the ground scatter has all died out again.

DR. BARNES. Thank you, Allen. Any more comments?

(No response.)

DR. BARNES. Dr. Millman has asked for a few minutes to present something, which I think is of importance to us as we listen to the people from the different organizations talk about their work, that is the philosophy that lies behind their approach, and Dr. Millman will tell us of the Canadian philosophy.

VI. The Work at Ottawa

Dr. Peter M. Millman
National Research Council
Ottawa, Canada

I wish to point out to those here what we have been doing in the past years and what we expect to do in the future. We have a comprehensive meteor program going in Ottawa at the National Research Council, and I realized in talking to some of those attending this conference that there wasn't a general knowledge of exactly what we are doing. We are not engaged in determining wind in the upper atmosphere. However, we may be now, and possible will be in the future, obtaining information that is of interest to this group.

We are essentially interested in the physics and the science of the meteor phenomena, and, starting in 1947, we observed in cooperation with the Dominion Observatory by radar and by visual and photographic methods. This was an extension of a program started in Canada (minus the radar) back in the 1930's. The program became consolidated at the beginning of the IGY when we built the Springhill Observatory for the purpose of having a center for meteor observations. We are operating the following two equipments:

1. A low-power meteor radar at 32 MHz. This operates as a patrol 24 hours a day with between 95 and 97 percent serviceability. Started in October, 1957, it has been continued from then up to the present time and will continue until the interference due to the new sun spot maximum becomes annoying, at which time we feel we will have collected enough data in this particular way, and will terminate the operation of this equipment. The antenna system is omnidirectional. I don't

think the other specifications are particularly important here. We have attempted
to maintain, as has Professor Ellyett for the southern hemisphere, a very constant
operating level. We have records of the various parameters and we have tried to
make it extremely regular in its recording of the echoes. The data is read from
film and each echo is recorded to the closest 10 km of range and in five categories
of duration, <1 sec, 1 to 2 sec, 2 to 4 sec, 4 to 8 sec, and ≥ 8 sec; and the results
are coming out in a series of papers by McIntosh and myself entitled, "Meteor
Radar Statistics." The second has just appeared in the Canadian Journal of Physics,
Volume 44, p 1593, 1966. We hope to have the third in the press shortly and there
will be a number of others. The peak power of this equipment is about 20 kw.

2. The second equipment we use is called a high-power meteor radar, which
operates at 32 MHz between 3 and 4 megawatts. It also has an omnidirectional
antenna, and was started in 1961. We had, prior to that, a 400 kw system. The
high-power system is used at the time of special showers and occasionally we run
it at the dark of the moon periods, probably at least 24 hours a month in addition
to special showers. The basic philosophy of operating this equipment is to corre-
late with photographic and visual recording, so we add meteor spectrophotography,
with 15 to 18 cameras operated at a time, and a team of eight observers in a spec-
ially constructed station to provide maximum efficiency of the data recording. We
have recorded about 30,000 meteors visually at Springhill, and for the IGY and a
period after that we collected another 100,000 from various amateur societies.
These data have been put on punched cards. We are now working up the data to
correlate the visual rates with the radar rates.

The omnidirectional nature of these antennas was chosen because most other
people were using some gain in antennas, and because we wanted to have the maxi-
mum correlation between the radar and the spectrophotographic and the visual
records.

Sometime next year the basic philosophy of this program will be altered
slightly, because we feel we have enough collected now on the patrol radar. We
have not fixed our future program definitely, but we talk in terms of stopping this
and perhaps starting another equipment, possibly in cooperation with the southern
hemisphere, but this is still in its very early stages of discussion. The changes
we would make would be towards more automatic recording; less reliance on visual
reading of the records, and, in the case of the high-power radar, the addition of a
bit of directivity to the system. Possibly, we will be reintroducing some cw ob-
servations, which were terminated after McKinley's large program of cw velocities
in 1950.

I should perhaps mention one other thing. In the 1958 to 1960 period, we set
up three stations. One station had 400 kw peak power, and the other two stations

had about 100 kw, and we observed radar echoes of bright meteors from three directions and computed heights. I think this information is significant because I don't think it has been repeated, and we did get a similar picture of the radar echoes from three directions, indicating that these detailed echoes had several echoing areas and were certainly reflecting back in three distinctly different directions in a similar way. The picture of a trail as specular reflecting mechanism is certainly not correct for the overdense, long-enduring trails. I think most investigators are aware of this fact now, but it did take a bit of missionary work to get the idea across, because so many people were using the directional antennas that only recorded meteors at the specular reflection point, or so it seemed.

I think that gives you a very brief summary of our past, present, and future activities. We certainly intend to continue the work in Ottawa in the meteor field.

DR. BARNES. Thank you, very much. Are there any comments? Bob Roper, maybe you would like to say something about your philosophy.

VII. The Adelaide Program

Dr. Robert Roper
Georgia Institute of Technology
Atlanta, Georgia

DR. ROPER. Comments on the future of the Adelaide program should really be made by Graham Elford, but I will try to present a summary in his absence. We have decided to repeat the shear work we did back in 1961, the results of which are in the process of publication (Atmospheric turbulence in the meteor region, Journal of Geophysical Research 71:5785, 1966). The three-station system has once again been set up, and we hope to continue with this aspect of the program on a routine basis, probably recording for several days at the beginning of each month, with occasional extended periods of operation particularly designed to look for synoptic disturbances within periods of several days.

We also have, as has been mentioned before, a large low-frequency array which will be used for D and E layer backscatter work. We are at the moment running a modified Mitra method in conjunction with our routine meteor wind observations to try and get some correlation between meteor winds and E-layer drifts. Unfortunately, so far we have not had both systems recording simultaneously, but will be doing so in the new year.

We intend to pursue once again the orbit work which we did back in 1961; this aspect of our program will be of interest to the astronomer. With the much higher echo rate we now have, we expect to get considerably more detail in winds, turbulence, and orbit distribution.

Another area I am interested in, but for which we are not yet set up, is the
measurement of time correlations determined from winds measured in a small
volume of the meteor region (narrow beam antennas) to try and determine the re-
lationship between the energy in the 1- and 2-hour period gravity wave phenomena
and the intensity of the turbulence. There is evidence of the gravity wave energy
being cascaded down into the turbulent shears, but as yet no direct determination
of this energy transport.

DR. BARNES. It sounds like a very ambitious program.

DR. ROPER. The program is just a reflection of the enthusiasm Graham
Elford has for this work, an enthusiasm he is able to pass on to his research stu-
dents and colleagues. In conclusion, I might add that we hope, as Graham has
already mentioned, to measure irregularities in atmospheric density using a laser
radar. We will attempt to run this experiment simultaneously with our other
programs.

DR. BARNES. Thank you, Bob. Gentlemen, it is almost 4:00 o'clock. I
think I will adjourn the meeting until 7:00 o'clock tonight, when we will have din-
ner for those who would like to join us.

(The Conference recessed at 4:00 p. m.)

Session 4

General Session

Chairman

Dr. Arnold A. Barnes, Jr.

I. Discussion of Joint Program for Geminids

DR. BARNES. The American Meteorological Society extends an invitation to any of you who would like to visit their headquarters at 45 Beacon Street in Boston. I am listed as the first speaker this morning. I had nothing prepared at the beginning of this week, but there are two points that we should consider.

The first is joint programs. The discussion that we had yesterday about the density and the fact that the Geminids produce comparatively uniform densities should be recognized by people who are trying to obtain densities. Now, I have asked Bob Nowak to operate during this period. AFCRL has a contract with Stanford and there is some flexibility in the dates, so he has agreed to take observations during this shower. Both AFCRL and Stanford will try to operate during the period seven through fifteen December, which includes the peak. The actual date is the fourteenth for the peak. Somebody mentioned last night at the dinner that apparently the small particles come in a day or so before the large particles.

DR. ROPER. A few days bef r

DR. MILLMAN. If you remember it showed on the diagrams yesterday from New Zealand and Canada. The buildup was quite broad.

DR. BARNES. We could possibly get some additional information out of this, knowing something about the range of sizes that are coming in. I looked up the declination and right ascension, which are 113 degrees and 32 degrees. This means that the radiant passes at about 2:00 a.m. local time.

Figure 1 was constructed to determine in what direction we should be looking in order to pick up the Geminids. Our antennas were supposed to be pointed at 270 and 360, but unfortunately the 15 degrees magnetic declination was put on the
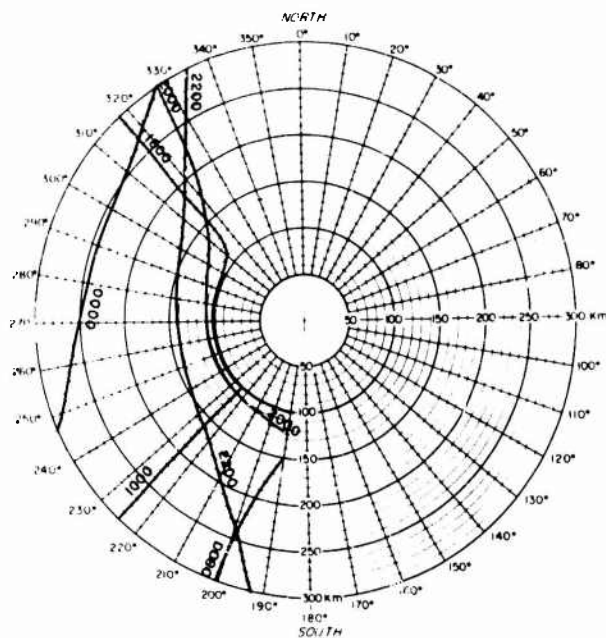
GEMINIDS   13-14 DEC 1964

Figure 1.

wrong way, so we point at 330 and 240, which for the Geminids is going to be an asset to us because we will be able to observe more of the meteors. The lines that are drawn on Figure 1 are the locus of the specular reflection points for meteors that came in with the particular radiant of the Geminids, and I assume that all the specular reflection points are at 100 km. The numbers are the hours in local time.

1 would like to ask the other groups if, perhaps, they could take observations during this time. We could, perhaps, look at the variations in the densities at different parts of the world. If the other organizations would be willing to join us, I think the meteorologists could learn a good bit on a global basis.

DR. MILLMAN. What is your reason for starting on the seventh and going to the fifteenth?

DR. BARNES. Well, first, the dates I obtained from Sugar's article on meteor showers, and generally we operate for only a week at a time.

DF. MILLMAN. Well, according to our records, I would say that you are cutting it a little short at the end of the shower in comparison to the beginning. 1 would shift it at least another day, but this may not be too important; then there is the question of whether you are talking about Greenwich dates. Then you are still within the showers on the 15th, according to the rates.

DR. BARNES. With sets that don't operate all the time, it is a question of getting them operating, so we try to start operating a few days beforehand, so that we know we will be on the air.

In fact, Bob Nowak and I were trying to start on the first of August. We at AFCRL didn't start until after the sixth, and Bob was a little later than that. My set had been off since sometime in May.

II. Discussion of Data Format and Required Rates and Accuracies for Wind Studies

DR. BARNES. The second point I would like to bring before this Workshop is the fact that we presently have wind data and more wind data will be available, and it should be put into the meteorological data banks. For instance people are trying to get Greenhow's data. Apparently Graham Elford has been successful in obtaining it, and it has been obtained by other people for special use. If this could be put into the WMO (World Meteorological Organization) and made available to all meteorologists, it would be a great help. Now, I realize that when you get data, you want to get all you can out of it first. But, there comes a point when you feel that you can release it to the meteorologists. For this reason I think we should approach the WMO with a data format of some sort, and ask them to approve the format for the recording of the wind and possibly the density data. This is something that we should discuss among ourselves in order to decide what information we feel should be given to the meteorologists.

Dr. Millman, was there a format suggested during the IGY? As far as I know there was no format suggested for winds.

DR. MILLMAN. I don't recall any in connection with meteors.

DR. BARNES. It was only in connection with rates and magnitudes?

DR. PETERSON. There was a little manual that was put together which included recommendations for meteor winds as well as fading rate.

MR. MYERS. You mean the IGY handbook?

DR. PETERSON. No. It didn't have a format.

DR. ROPER. What about the IQSY, they have a number of handbooks there.

DR. BARNES. This is in the IQSY manuals?

DR. ROPER. Yes.

DR. BARNES. We will look into that, and we will see if there are any suggestions or recommendations we can use as a starting point.

DR. ROPER. I don't know if there was any suggested format or anything like that.

DR. SOUTHWORTH. I am wondering if we really want to set a format. I have tried to work with the IQSY cover format, and I think it is an absolute horror. The people who thought up that format had no idea what might be my observations and what they were going to be like. It is entirely inappropriate. I have miles and miles of nonsense.

DR. PETERSON. No, no, we are suggesting that you help set the format.

DR. SOUTHWORTH. Yes, but somebody else could have the same problem.

DR. BARNES. This is the reason that I think we should discuss the problems here and now.

DR. MILLMAN. One should be familiar with what has gone by in the IQSY and the IGY. Some of the things that were proposed for the IQSY were horrible. In the aurora field, I certainly cannot agree with the way they set it up; it was horrible.

MR. MYERS. Isn't the only answer to that to set it up yourself?

DR. BARNES. Certainly we don't want somebody else to do it. We would like to do it here and then go to the WMO with our format.

DR. MILLMAN. But take account of all the thinking that has been done before.

DR. SOUTHWORTH. Equally, it should not be something that is set forth on a piece of paper, because this again is something that is machine output in a lot of cases.

DR. BARNES. I would suggest cards. This is what Bob and I agreed as a working format. There is redundant data on the cards. We are trying to cram as much on to each card as we can.

DR. ELFORD. You have to beware, of course, in setting it up that it is readily interpretable by other people, for example, meteorologists, who look at this set of information and will read it with the right interpretation.

DR. BARNES. This is correct. I have the contacts with the meteorologists and after we have juggled things back and forth, then I would go to the people who are actually thinking of working with the data and discuss it with them and get their feedback. I have already discussed this with some of these people and I know what they are looking for.

DR. ELFORD. It would seem to me that probably the proper approach would be to ask the meteorologists how they would like to read out this information, and then where you are producing it, to see if you can match it up with this sort of an approach. Then we could come to some compromise.

DR. BARNES. We want to determine what they want out of the data because everybody wants different information. They want it in this column instead of that column, and I take it that we can ask them what they want and make sure that this information is available. Then we can set up the format. Let them change their programs to correspond with our format, but supply them the basic data.

DR. MILLMAN. It doesn't matter what is in what column, as long as everybody puts it in the same column.

DR. BARNES. That is the important thing, that we get the important parameters on the cards.

MR. NOWAK. You can change the format of a card in an automatic translator. It is such a quick job and it doesn't cost an appreciable amount of money.

DR. SOUTHWORTH. The other problem is, that when one is cramped for what will go in a card, and people make different choices, both sets of choices will fill the card. You cannot find one format to be suitable for everybody. It is likely to be that sort of thing.

MR. NOWAK. There is still the possibility of using two or three formats and preceding them in the first column of the card with an identification.

DR. SOUTHWORTH. Yes, we can do all these sorts of things.

DR. ROPER. Could we clear the air a little bit, Arnold? Are we talking about something which is coming out of an echo or echo pulse, or are we talking, in fact, about reduced meteor winds?

DR. BARNES. Well, knowing the problems that the meteorologists are trying to tackle, I suggest that they come out with one card for one meteor.

DR. ROPER. You can't do it.

DR. BARNES. Why not?

DR. ROPER. Because of the statistics involved.

DR. ELFORD. If you get one per hour or a thousand per hour, you can get very different interpretations on it. You would have to have such a large expanse of volume to go with this data.

MR. MYERS. What you are saying then is that you would prefer, perhaps, some time period summary with a number of observations and a number of trails to make this up?

DR. ELFORD. I think this would be a more appropriate way of doing it.

MR. MYERS. The meteorologist doesn't care how he gets the data.

DR. BARNES. But what about computing transports? You can't do that by means, because by the time you get to the means, you have already thrown out the terms that you are looking for.

MR. MYERS. The very moment that you say you will furnish information on a trail by trail basis, you are guaranteeing that this data is of sufficient accuracy and stability that it can be used for transport data. I think that is what the prime

objection is People really aren't sure that any one wind of any given trail is meaningful in terms of mass transport, for example. I think the people making the measurement have some reservations about it. At least, this is the feeling that I get.

DR. MILLMAN. My feeling, definitely, after hearing this presentation in this conference, is that at the present stage of the game it would be very dangerous to give raw data per trail to the meteorologist. I think that the person who is doing the experiment is the one who should interpret the data on the wind. I say this is my feeling. We are not working with winds, but I do know meteor trails, and I would hate to give any of our raw data without some guidance as to its handling. If you get data coming from various sources, I think you are asking for even more trouble.

MR. MYERS. You can compromise to some extent, it seems to me, between what the meteorologists can use the data for and what the data acquired is useful for. You may be competent, perhaps, in the other's field to some extent. But you could make a compromise by picking a data reporting period short enough to be satisfying to the user looking at fluctuations in the data. This period then becomes the reporting time unit (perhaps 15 minutes). You also report the number of trails in each period so that the unit periods can be combined into hourly, 3 hourly, or any manageable unit of time most useful to the data consumer. This gives the user some idea of the reliability of the data.

DR. PETERSON. Is there any reason to think that meteor observations are any more hazardous to give to people than data on the ionosonde? People have made use of this information for a long time. I wonder if we are being a little overcautious.

DR. CHAMPION. On the ionosondes, yes, if they have C-4 equipment.

DR. ROPER. But, you have to think of the number of ionosondes. Nobody has the same meteor equipment. I mean, these people out in Illinois are getting thousands of observations per hour.

MR. MYERS. That doesn't say anything about wind data.

DR. CHAMPION. You're not even looking at the same meteors in that case.

DR. PETERSON. I assume we are not. But do you really think it is all that bad?

DR. ELFORD. I think at this point in time it would be unwise to give in any more detail than the summary by hour or hour-and-a-half periods.

DR. PETERSON. All right, I thought you were suggesting that we should not give any data.

DR. ROPER. I think, of course, that everybody will have cards with this information on it. This is the raw data. This is going to be of use to the various

groups represented here and other groups in the field, but this isn't information that you pass on to a user.

DR. PETERSON. Well, it seemed to me that somebody was saying yesterday that they were very interested in this increased rate in the summer of 1963, for example, and that kind of data available at other places should be valuable as long as the equipment hasn't varied too drastically.

DR. ROPER. There is a difference, I think, between a meteor group exchanging information and something which we are going to hand out to meteorologists.

DR. BARNES. This apparently has split into two problems. The first is the information for the meteorologists, and I think it is the consensus that the mean data should be supplied to them rather than the individual trails, ---

DR. SOUTHWORTH. Perhaps I should make a comment on the Havana data. We have already agreed to give individual meteor data to---I don't know the organization, but it is in AFCRL.

DR. BARNES. This is Al Cole's group.

DR. SOUTHWORTH. Yes. So that certain data we think is good enough on individual meteor data basis; but there is an awful lot of it, and we haven't yet figured out how we are going to get means out of it.

DR. ELFORD. Doesn't this underline the point? If he has a system in which he thinks that he can hopefully get true wind components off of one echo trail, nobody else is in this position. His data is unique to his system. It is different from all the data reflected by other people.

DR. BARNES. He only has it off a plane of one meteor. He only has two directions in a particular plane. The plane is normal to the trail.

DR. SOUTHWORTH. Most of it is on the assumption of horizontal wind. We will get estimated values of vertical components.

DR. ELFORD. He is putting it into the cells, also, and you and I can't put it in separate cells. This is different from what anybody else is doing.

DR. BARNES. But I think there is more detail in this than there necessarily should be supplied to the meteorologists. I have talked to Al Cole about this and perhaps the thing to do would be to take the average throughout a particular layer, whether it is 1 km or 2 km and use this and average over a time period.

Well, I think this is the first problem, the one that I was really addressing myself to, that we can supply mean wind data to the meteorologist. This is something that we should go forward with to the WMO.

Now, as far as interchange of data among ourselves, because of the variations of the different sets and the type of information that is obtained, we really shouldn't set any standard format at this time. You see, Dick Southworth has a lot more information than I would obtain and he would probably fill up two or three cards for each trail; whereas, I have all my information on one card. I don't think that we

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should try to standardize at this point, but let everybody go his own way in recording and reducing his data. If you want to use his information, get his interpretation as a guide.

Do you gentlemen think that I should approach the meteorologists as to a standard format?

Do you think that we should get together to discuss a standard format for mean winds?

DR. ROPER. You will find a recommendation to this effect in the WMO Technical Note No. 58 by Haurwitz. Here the meteorologist lays down just what the meteorologist wants in terms of the sort of things we are talking about, prevailing winds and tidal components in particular. He is very conscious of errors. He wants an estimation of the errors. (Tidal Phenomena in the Upper Atmosphere, Technical Note No. 58, B. Haurwitz, WMO-No. 146 TP 69, 1964.)

MR. HERING. This is sort of a lengthy problem to resolve this morning. I suggest you take on the job of the format and send it around to these people.

DR. BARNES. I just want to bring it out into the open at this point so you can think about it. Wayne, you have been through this with the ozone?

MR. HERING. Yes. You run into problems because you have experimental devices and it really requires an explanation as far as meaningful data are concerned. But you are converging on a proper answer to provide mean data over a specified time period, as long as you mention the reservations along with it. I think this is the best you can do. I think you can prepare that format.

DR. CHAMPION. One aspect is the altitude and the error with the wind shears. If, for example, this is compared with chemical releases or other techniques, the accuracy is important.

DR. BARNES. That is correct; we can put only so much information on the card.

MR. MYERS. If you can't put it out there, there's no sense to put it on the card.

DR. ROPER. It is basically by the time scale.

THE FLOOR. From one piece of equipment to another it varies.

DR. CHAMPION. Are you suggesting an average over, say, 15 minutes? Then essentially, it is going to be an average over one kilometer. You have to specify a box like that.

DR. MILLMAN. Wouldn't it be a good idea while everyone is here together to get a consensus as to the quantization in time and elevation as a guide.

DR. ELFORD. I have drawn up a diagram which I can put on the board directly along these very lines. It will give you some idea of the critical measurements we should go into. I'll put that on the board now.

DR. SOUTHWORTH. It is a great deal more trouble to requantize somebody else's data.

DR. BARNES. I think as far as the time interval, it would depend quite a bit on the equipment.

DR. ROPER. Of course, the time interval would be determined by the rate.

DR. BARNES. Yes, here I am depending on my threshold level, whether it is 30 an hour or six an hour.

DR. ROPER. And you get this sort of diurnal variation anyway.

DR. BARNES. Yes.

DR. MILLMAN. I have had some experience in trying to iron things out like this for the IGY in connection with the aurora program, and I think that one should be flexible, but it is, also, ultimately important to decide on the basic units which must be depicted in time, and then those could be combined, if necessary. It helps a lot if you can get everybody working on the same basis. If you are doing an hour, say you start at a half hour and tabulate the results for a period of 60 minutes which centers on the hour. This, again, can save a tremendous amount of trouble later on when you put data from several groups together. By looking from the outside, I would think that one of the things to be done this morning is to get a preliminary estimate as to how people want to divide it up. Then you should leave some flexibility. If you have only a small amount of data, you can only get small amounts of information; if you have more data, you can break up the hour. Where are you going to break your hour, that is the point.

DR. BARNES. I think that Graham has a handle on this. He was telling me this morning that he was looking at some of the questions that were going to be answered by the data.

MR. MÜLLER. In the IQSY recommendation it said that we want to submit hourly values of the total magnitude and the direction of the wind. This is all. This is probably not enough for our purposes, but it might be possible to include something like this for a brief reference.

DR. BARNES. There is the question of height resolutions that can be obtained by the various sets. We would like to have one kilometer intervals.

MR. MYERS. In other words, heights presented to the closest kilometer, plus or minus whatever error is indicated in that particular set of data.

DR. BARNES. Then there is the question of whether density should be included. After yesterday's discussion, maybe we shouldn't provide density.

MR. MYERS. You can make provision for it, but you don't have to fill in the column.

DR. ROPER. It would really be better to use diffusion coefficients. Because, after all, it depends upon which model you are going to use. We worked with this

on the chemical releases and I think it is valid to give diffusion coefficient which is something you observe. I don't think we really know how to give the proper density yet.

DR. PETERSON. It is better to give decay rate, then.

MR. MYERS. This is the thing you actually observe.

DR. ELFORD. I dreamed this (Figure 1) up practically after breakfast. The idea was to try to identify what is the critical measurement that we should be considering in our equipment. So, I have in the first column written down what I would think, subject to correction, is the main wind component that we can identify at this point in time. I called it the prevailing component which is independent of time. The next group is planetary and tidal waves, mainly identified by the periodicity down to a rate of six hours. Finally, in the last group, the residual gravity waves to the gravity wave people or turbulence to the turbulence people.

From our measurements at Adelaide we would put down for the height gradient (which is the next piece of information) the value of not greater than five meters per second per kilometer for prevailing wind. There are many mixed up units here, but this is an easy one to identify in terms of effective error of one kilometer. Maybe I should just ask for your comments as we go along. Is there anybody that would like to criticize this one?

(No response.)

All right. From our Adelaide work we would identify a height gradient in the tidal wave component of not more than four meters per kilometer, including the effective phase for change in height.

DR. PETERSON. How do you pick the numbers? Do you think we can measure that or do you think that is what is important?

DR. ROPER. We think this is what the wind does.

DR. ELFORD. We measured this and we think we have sufficient resolution in our data to identify this as a mean quantity.

DR. PETERSON. You say two things; you say this is what you think, and this is what it is. I was wondering which one was the most predominant?

DR. ELFORD. We think we have sufficient height resolution and rate information to identify this point.

DR. BARNES. We are really interested in an upper limit at this time.

DR. ROPER. It is much greater than one and less than ten--let's say less than five. We don't measure over a long period of time.

DR. ELFORD. We c identify with our equipment gradients of ten meters per second (per km.)

DR. PETERSON. Is that also true for the chemical releases?

DR. ROPER. It is the right order of magnitude, yes!

WIND COMPONENT	HEIGHT GRADIENT	TIME PERIODICITY	ΔV FOR ΔH=1 km	CRITICAL MEASUREMENTS	MINIMUM ECHO RATE
PREVAILING ‒ • ‒ MEAN	≤ 5 meters per seconds per km	∞ 3 or 4 DAYS	≤ ± 5 m/sec (5-10% ERROR)	HEIGHT TO 3 or 4 km	COUPLE PER HOUR
PLANETARY WAVES ‒ • ‒ TIDAL WAVES	≤ 4 meters per seconds per km	24, 12, 8 6 HOURS	≤ ± 4 m/sec (10% error)	HEIGHT TO 3 or 4 km TIME TO 15 - 30 MINUTES	20 PER HOUR
SHEARS ‒ • ‒ TURBULENCE ‒ • ‒ GRAVITY WAVES	≤ 50 meters per second per km (RMS of 15 meters per second per km)	0 2 - 5 0 HOURS	≤ 50 m/sec (50% error or greater)	HEIGHT TO 0 5 km TIME TO 5 MINUTES	HUNDREDS PER HOUR

Figure 1. Wind Studies and Radar Meteor Trail Measurement Requirements

DR. ELFORD. When we come to the turbulent component, I used the French data because I think they have the height resolution better than anybody else. I think this ties in pretty well with the chemical release observations.

MR. MÜLLER. We observe up to 50 (m/sec/km) with a definite cut-off. There is a definite response up to 48 or so.

DR. ELFORD. What about the chemical release people; do they like 50?

DR. CHAMPION. That is quite a large number offhand.

DR. SOUTHWORTH. Certainly some of the photographic meteors show a great deal more than that.

DR. ROPER. Not below 105 km.

DR. SOUTHWORTH. Yes, these are long enduring trails.

DR. MILLMAN. There are very few above that, but most are below that.

DR. SOUTHWORTH. There are occasional kinks, but they are very sharp. I just go down one trail and I find a short ridge in it, so that perhaps if you were observing only a single plot on the gradient train, from this photographic train, it is not uncommon that somewhere it will be a very high shear.

DR. ELFORD. Well, we have to put some figure in here.

DR. BARNES. But we want a reasonable upper limit.

MR. MÜLLER. These results are based on a year's average of all the data. If you do get a response above 48, you may as well say 50.

DR. ROPER. Could I show a slide (Figure 2), which summarizes the vapor trail work? It is Figure 7 of Adam Kochanski's paper "Atmospheric motions from sodium trail drifts". Journal of Geophysical Research 69, 3651, 1964. This particular slide, although it contains data from only 25 firings, does span most seasons.
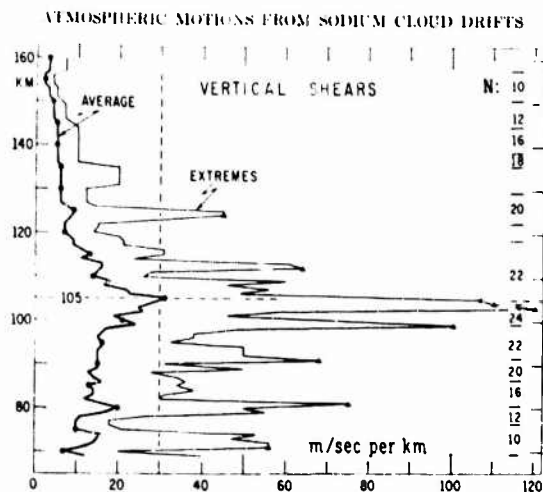
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Figure 2. Vertical Wind Shears Based on Sodium Cloud Data From Wallops Island. Vector shears were computed for $\Delta z = 1$ km and plotted for each kilometer of elevation. N is number of observations

There is a limitation in the data in that the shears were measured only at twilight. I do not believe, however, that inclusion of data from other times of day would modify the picture significantly. Gradients of 100 meters per second per kilometer are not common, but are measured occasionally.

DR. PETERSON. Then this is the period (twilight) that Revah's records showed as the maximum, wasn't it?

DR. REVAH. It was during sunrise. The maximum is after twilight.

DR. ROPER. Dusk is the period of observation. We have found no meaningful difference between a.m. and p.m. twilight.

DR. ELFORD. You can identify this in various ways. You can say that 90 percent of the shears are going to be less than 40 m/sec. That is the only way to put limits on this thing.

DR. ROPER. Unfortunately, I don't have any information as to how many times these values exceeded the maximum. You can see the RMS values of 15 m/sec/km averaged over the meteor region.

DR. ELFORD. Now we put in the time periodicities. We identified the 24-hour, the 12-hour, and the 6-hour data as tides. There are other harmonics, too, but these are the predominant periods. For turbulence, I think, the lower limit we should have is 0.2 hour which is probably a reasonable one for the time periodicity in this region.

MR. MÜLLER. Possibly more than two hours for an upper limit. Something like four and a half hours. I observed four and a half hours.

DR. ELFORD. Let's put five in as an upper limit.

MR. MÜLLER. May I please come back to the previous line, planetary time waves? Have you considered the possibility of a slowly varying component which also has been reported by Greenhow et al, which may last a period of a few days. This is a big concern still, but this may be a genuine periodicity. I am going to talk about this a little later on.

DR. ELFORD. You can put it in there if you like.

DR. ROPER. It is probably a synoptic variation.

DR. ELFORD. This is what I am trying to get out of the table, the error in the wind velocity for a height error of one kilometer. Now the prevailing wind has an average speed on the order of 50 to 100 meters per second. This depends upon the height and the time of year. So, this probably represents something on the order of 5 to 10 percent error when you observe wind, if you got that height error. The same applies with the planetary tidal waves, probably starting at the lower amplitude of the wave. So, maybe there is a 10 percent error.

Now, this turbulence is critical. Here you could have an error of ± 1 km, and have an error of 50 m/sec, or 150, whatever figure you take. The wind speeds are on the order of 100 m/sec, so here you can get fifty to one hundred percent error. This we might be concerned with. That is how I tried to identify critical measurements.

As for the prevailing wind, I should think the height error of 3 or 4 km is still going to give you a good measurement of the prevailing wind. You are going to get near to 30 to 50 percent. It is at this time, I think, reasonable.

DR. CHAMPION. You have three or four kilometers there, and yet you have wind shears that are maybe a fraction of a kilometer, and I am not sure that I would drop those down to the last column.

DR. ELFORD. I don't quite follow you, Ken.

DR. CHAMPION. Wind shears occasionally are as much as 180 degrees reversal in less than a kilometer or maximum of a kilometer, and I don't think your data is meaningful with that large an error in the altitude.

DR. ELFORD. These are the wind shears down at the bottom of Figure 2. This is where the extreme shears are considered.

DR. CHAMPION. This is what I am doubting. I don't think that we can drop the wind shears down into the gravity wave-turbulence section.

DR. ROPER. It doesn't exist for days; it doesn't even exist for hours.

THE FLOOR. It can exist for hours.

DR. ROPER. A few hours.

THE FLOOR. That is right.

DR. ROPER. But that is the bottom box.

DR. ELFORD. This isn't something that will exist for days. This is the critical entry: infinity or three or four days for the time periodicity of the prevailing wind.

DR. CHAMPION. I agree with you in having the time scale at the bottom, it was just that I wasn't sure of thinking of wind shears in terms of gravity waves.

DR. ELFORD. Well, maybe you can't break it up into all these sections. Maybe this is too artificial. It is just a method that I put forward as a suggestion to help us analyse the facts.

MR. MYERS. I think this will sort itself out when you convert it from minutes to quantization. This is what you're going to end up with in your data.

DR. ELFORD. Right. This same argument applies to tidal waves. The critical measurement in height is probably 3 or 4 km; and you require time of 15 to 30 minutes.

With shears, obviously, one can't have much more than half a kilometer, because that is the length of your Fresnel zone. So, you get the smoothing of this component just due to the measuring technique.

DR. PETERSON. You are going to get smoothing of sorts as the center of gravity, if you will, or something smaller than that, slides along the trail.

DR. SOUTHWORTH. The essential point here is that if you have these shears, (which I don't think you are ever going to be able to compare so that you will know within 0.5 km where they are) you want to know what magnitude of shears you have. You don't know exactly where they are.

DR. ROPER. That is correct. There are cases where you want to know height within 0.5 km or 1 km; for example, we are doing work with our spectrometers and we know within 0.5 km where they are. We get particular changes, a quite important aspect of them.

DR. SOUTHWORTH. All right then, there are exceptional cases.

MR. MYERS. That isn't suitable for this at this time.

DR. CHAMPION. Yes, we are using chemical releases. It would be nice if we could use the meteor station; it would be cheaper. I mean, this would be a nice application.

DR. BARNES. This is a different problem, and we are getting away from the problem to which we are trying to address ourselves.

DR. ELFORD. Well, probably times of five minutes is all you would require in the block for critical measurements of shears. Anybody like to critize that?

(No response.)

Well, then, finally I would think that somebody should try to put in some things for the echo rate. I finished breakfast and left it vacant. Maybe somebody would

like to fill this in? (The last column, Minimum Echo Rate, in Figure 2 was supplied by the editor.)

DR. BARNES. Thank you, very much, Graham. I think that from what you have presented we can achieve this, and we can now readdress ourselves to the problem of providing information to the meteorologist. It is this type of information that the meteorologists are interested in, and it is perhaps this type that we should think about supplying to the meteorologist.

DR. PETERSON. Which one?

DR. BARNES. The information on the Planetary and Prevailing winds. Now, we have critical height intervals of 3 to 4 km, which can be achieved by our sets. Can you achieve that at Sheffield?

MR. MULLER. Yes.

DR. BARNES. And the French system and obviously at Havana and Stanford. We have the time interval, 15 to 30 minutes. The question now is, can we obtain enough information in these time periods. This depends upon the echo rates and Graham hadn't gotten this far with his diagram.

MR. MYERS. You are asking, can we achieve a 15- to 30-minute value? What is meaningful is, how many observations provide a statistically meaningful average for any given time period? You set some cutoff rate for the numbered usable echoes you observe in a period, and if your set doesn't produce these echoes in an hour, then you are not going to get any meaningful information out of it.

DR. ELFORD. I don't think that anybody has yet done this sum with this statistical set. It depends upon how many hours you require, the gradients in height, and resolution needed to identify a particular periodic component.

DR. PETERSON. I would say several hundred is enough.

DR. SOUTHWORTH. The Groves analysis deals with several days. If you don't get enough echoes per day, you take more days.

DR. ROPER. This relies upon the stability with time. If you have phase changes, then you are in trouble; but in general, I would think that our results show that you can measure upper air winds over a 10-day period. They are changing in this time, but the mean value is probably quite meaningful. As far as the time components are concerned, you are better off there, because there phase stability does seem to be useful in terms of a low hour-by-hour basis. There is phase stability. You still do get an average for several days, and you get a nice spectral peak for 12-and 24-hour components.

DR. ELFORD. We find that we can get components with reasonable errors, say 10 to 20 percent errors, using the Groves analysis and extracting prevailing and tidal waves for one day's echoes, if you have something like 400 echoes in that one day, as long as your diurnal variation in rate does not vary by more than a factor of four all day.

THE FLOOR. I am not sure that that five-hour upper limit is a safe one for shears (Figure 2).

DR. BARNES. Should it be larger?

THE FLOOR. Well, we found an indication of 10-hour variations and we certainly have seen over longer periods than five hours a similar pattern of the detailed long-enduring echo. I am not saying it is, but I suspect that that may not be long enough; that you may have some periodicity longer than five hours.

DR. PETERSON. Once again, we are not going to throw out something that is 5.1 hours, are we? What is this table for? Is this just a guide?

DR. ELFORD. It is just a guide; just some figures to think about.

MR. MYERS. It is really the most useful tool you can have for determining quantization levels for your lower limits, your fineness of resolution, and for which you believe your data is meaningful. It may be advantageous possibly to have your smallest time quantity meaningless, so that the people can see that there is a lower limit in the data.

DR. ELFORD. The velocity quantization here is much cruder than the effective height quantization, and the height quantization is the critical one. There was some discussion on this the first day, viz., how accurate you can measure the Doppler frequencies. When you look at this sort of table, you can appreciate that this is probably not the critical parameter at all.

DR. BARNES. I feel, in looking over these figures, that data presented to the meteorologist should be two-hour averages.

DR. PETERSON. If you are going to do that, why not just settle on one?

DR. ROPER. This ties in better with the present method of representation. I think most of us, if we were looking for prevailing tidal wind, average for an hour.

MR. MYERS. I think if we go to two hours, we have to start on odd or even.

DR. PETERSON. Even for one hour, you have to decide whether you are taking observations between or around the hour.

DR. ELFORD. What is the normal meteorological approach on the interval?

DR. BARNES. For upper air data, we are lucky if we get it every three hours. It is usually once or twice a day.

MR. MÜLLER. In our case, you could say since you must be working in two directions, take a bit before it and a bit after it, and concentrate on the hour.

DR. ROPER. So it is agreed that we average the half hour either side. This isn't what we do at the moment, but we will do it that way.

MR. MYERS. In the upper air data, you refer to the hour by taking the closest half hour.

DR. BARNES. But it takes a balloon so long to rise.

MR. MYERS. It takes us so long to gather enough echoes, so it is comparable.

DR. BARNES. So we take the information centered on an hour, a half hour on either side.

DR. ELFORD. Right.

DR. ELLYETT. This is Universal Time, isn't it?

DR. BARNES. Yes. We don't want it to be on the quarter hour or something like that.

DR. MILLMAN. It should be Universal Time. That would conform to other upper air information like aurora data for both the IGY and the IQSY and to other information tabulated across the world.

DR. ROPER. Dr. Millman, aren't we confusing astronomical and geophysical formulas here? The total oscillation is important in terms of local mean solar time. Everybody was to get out solar and tidal data, and they would have to transform Universal Time into local mean time for comparison purposes.

DR. PETERSON. Is there a possibility of putting both on? Is that too much trouble? Sometimes it is hard to tell where you are.

DR. MILLMAN. In the auroral data, it is very definitely controlled by the local time, also. I wasn't making any suggestion one way or the other. I understood people to say that they would do it on the hour, and I merely commented that this would then agree with the world agreement on auroral data. Now, I wasn't making any suggestion that that was right or wrong. I merely stated that in both the IGY and the IQSY we have had world collection of auroral data based on the hour. That is, the first priority is to give what happens at zero minutes of a universal hour. Now, as I say, there is certainly room for argument whether that is right or not.

DR. PETERSON. Well, the kind of time you (Dr. Roper) are suggesting is mean solar time?

DR. BARNES. Well, I disagree. It is the meteorologists that want to use the data, and they look for other things that appear besides the tides. The tides can be taken out, but they will also be looking for the synoptic features. Greenwich Time or Universal Time, as it is now called, should be the one that is used. It will also eliminate any confusion when you get to the Australia data as to whether this is really the same day as the information from Stanford.

DR. ELLYETT. I would say that all ionospheric information is in this form. If it is in Universal Time, then it can be changed into what one wants; but if it is something else, you will have a job sometimes just to find out how to put it into Universal Time.

DR. ELFORD. I will go along with that.

DR. BARNES. I think that since we are talking about supplying it to the meteorologist, we should use Universal Time because they are used to using Universal Time.

MR. HERING. Another type of information that you are getting from the meteor which you might find desirable, is the shear information obtained from two measurements on the same trail. I am very much interested in this. I found out about it the first time in the session here. I must say, though, that I am completely perplexed at this point. The data presented on your slides yesterday showed the variation of wind over one kilometer. That does not at all reflect the extreme variability that was talked about in Figure 3. Is there some filter passed between these?

DR. ROPER. These were averaged over ten days. If you measure a shear over a certain height range, you have one echo, another echo, another echo, another echo, and so on.

MR. HERING. These are all pairs?.

DR. ROPER. All pairs, and they occur at different separations each time, and you set out a series of values, and all you do is set up a series of boxes and into each box you put the value of the velocity that was measured. On the slide I showed, 15 m/sec/km is the average shear measured on a sodium trail at twilight and that seems to be one of the disturbed periods. I got the highest shear I measured at Adelaide in September, one of the disturbed months, where in fact it was 9 m/sec/km.

THE FLOOR. I was completely misled by the recent conversation.

DR. ROPER. Well, these do exist, but they are not the RMS shears.

DR. ELFORD. This is another one of those extreme and means problems. We should have another line, which is just shears.

DR. BARNES. So your suggestion is that the shear data also be included in the data provided on cards.

MR. HERING. If possible.

DR. PETERSON. I think it also has an additional meteorological use and it also has geophysical use in terms of things that happen to shears.

DR. SOUTHWORTH. I expected to get a lot of data and I certainly haven't figured how to put it all out yet.

DR. BARNES. This is an important point, that beside the winds, we should give the shears.

DR. ELFORD. There is the other shear information that you get from the change of Doppler frequency.

MR. MÜLLER. I just looked at some of my records, and I found that we tried to investigate this further. You could probably do the same.

DR. REVAH. Don't shears enter into the gravity waves? They are not something apart.

DR. PETERSON. What's wrong with labeling the last row of Figure 2 gravity wave turbulence and shears?

DR. ROPER. Shears covers the lot.

DR. BARNES. To return to our problem, we will supply winds and shears. The other information that we could supply to the meteorologist would be decay rates; not density, not diffusion, but decay rates.

MR. MÜLLER. You mean decay time constants, to be specific? On your records it is just a slope.

DR. BARNES. But if they want to interpret this, they have got to know the parameters of the equipment, and wavelength enters into this.

MR. MÜLLER. That is right.

DR. SOUTHWORTH. Isn't it fair to give diffusion rates?

DR. PETERSON. It was until I saw Graham's data.

DR. ELFORD. I don't think we have enough knowledge of the physics of this yet to make the step from decay to diffusion.

MR. ZIMMERMAN. May I make a suggestion concerning shears which would be of use to the meteorologist and people who study dynamics in this area? Determine the shears with the components that you are looking at, rather than the total shears, because there is some reason to believe that the 24-diurnal component is the main source of turbulence around 90 to 95 km region. Thus it is possible to break up the components of shears and have the individual components.

DR. ELFORD. I don't see how you could do this.

DR. PETERSON. Neither do I, but I think we can give him what he wants under a different category, because we are going to give him the wind versus height.

MR. MYERS. He came in late, that's the trouble.

DR. ELFORD. Aren't we really saying that we are going to identify two sorts of shears? We are going after 5 m/sec/km. These shears are probably going to be contained in the prevailing tidal waves. From there on, the high shears we can put on the bottom line.

MR. MÜLLER. There is just one point associated with this. If you use two aerials, you get two lots of information.

MR. MYERS. What he's really saying is that we have two things that we are really talking about. One is differential winds that you can get from height versus velocity data, and the second, instrumentally determined differences, individually measured shears.

DR. ELFORD. That is right.

MR. MYERS. These are two different kinds of information.

DR. BARNES. The question is then are they equivalent?

MR. MYERS. They may or they may not be, but the greater probability is that they are not equivalent. This is for the meteorologist to determine.

DR. ELFORD. What we are really doing on the last line of Figure 2 is to identify shear on a single trail. We also get shears computed from several trails at different heights and slightly different times.

DR. PETERSON. I don't quite understand that, because on the previous slide (Figure 3) we don't have any short time intervals at all. I don't really think that all the short term things will be identified by single trails.

DR. CHAMPION. It is a question of statistics. I mean, this is the shortest interval, and you have means from a number of trails. It may be that one set of equipment might get these measurements from one trail.

DR. PETERSON. It might happen to more than one trail.

DR. CHAMPION. Yes, sure. Before you can get mean values and extremes, obviously it counts one trail, and you should have more.

DR. PETERSON. But you get up to several hundred per hour in the early morning hours on some of the equipment.

DR. CHAMPION. It is a function of altitude, too.

DR. PETERSON. What I am saying is there may be short time durations observed not on a single meteor, but observed on a large number. But you don't want to average out over two or three hours and then say that there isn't any short term duration.

DR. MILLMAN. I think the confusion arose because one meteor was mentioned in connection with shears and according to a strict definition you can't determine a shear except from one meteor. Now, you may have shears determined from a number of meteors, but the individual shear cannot be determined unless you know your differential observations on a single trail. With an accuracy of 3 or 4 km in height, you can't get the information to determine that shear. Wasn't that the argument, that you have to have the two measured on a single trail? You just don't get the shear unless you do make two measurements on one trail.

DR. PETERSON. Yes, but you will get short term variations using two or more trails.

DR. BARNES. You can obtain actual shears in either of two ways. You can get it from the two points on the trail or you can get it from a change in the Doppler.

DR. REVAH. When I was speaking of shears for a short interval of time, I was thinking of the following picture. During ten or 15 minutes you can have several meteors occurring in an interval of height which may be 10 or 15 km. Then you are perhaps able to have enough accuracy in height to be able to reduce the wind during this interval of time and to be able to take the shear off this special wind profile in 10 minutes or 20 minutes. We have obtained several points from which (by linear interpolation, which I showed you yesterday) we tried to draw the wind profile. The wind shears I was talking about were obtained by taking off the

mean value and looking at the wind shear. It wasn't the wind shear obtained from a single trail at two different points.

DR. ELFORD. This is the point, isn't it? He has a system which has much better height quantization.

DR. REVAH. But it is only for one component of the wind velocity.

DR. PETERSON. Well, I think it is still possible, using a num.. r of meteors, as you described, that one can get shorter term variation than the tidal component; and if you are getting shorter term variation, that is interesting.

MR. MYERS. You are talking about something shorter than one hour? You have mentioned 15 minutes.

DR. REVAH. Well, that is what we are trying to do. Several times we have looked at the time variation of these profiles, and we have seen several profiles with more or less the same shape going downward in successive 15 minutes. That is the aim of our experiment: to look at the fine structure as a function of time of these wind profiles. In fact, we have to improve our height accuracy if we want to do something which is meaningful.

MR. MÜLLER. I would like to say that such exercises can be carried out occasionally (e. g., during major showers) but not all the time; so I don't think it would be worthwhile to include it in a general pattern of an interval of 15 minutes. You probably could do this ten or so times a year. Wouldn't you agree? You can't do continuous studies on wind profiles at intervals of 15 minutes.

DR. BARNES. I think we are getting away from the problem, viz., what we want to supply to the meteorologist. We have settled that we want to supply the average wind over an hour's period. The question is now about shears. There are three ways of getting shears. First, one gets them by taking a number of observations at different heights over a short period of time. Second, one gets them by taking two points on one trail. Third, one gets the shears using the Doppler shift from an individual trail. However, these shears are not equivalent, because in the case of the shear computed from the Doppler shift of a trail, one assumes that the trail is coming in nearly vertically. One doesn't necessarily know whether it is vertical or not. What one gets is a shear, and it is not necessarily a vertical shear. So, there is a question whether the three types should be lumped together. I don't think they should.

MR. MÜLLER. Then, I think, which way you derive this information should be mentioned; but I think the method is safe just the same. If you look at low elevations, for instance. and you take an average between five and ten echoes, then, I think you can assume the methods are equivalent.

DR. BARNES. To summarize, it looks to me as though the method of using the change in the Doppler is not equivalent to the other method. I think that perhaps this point should be put aside, and this probably should be broached to the

meteorologist at this time. If you do use the information from the individual trail, you are now getting back somewhat to supplying individual trail information.

MR. MÜLLER. You have to take means. The things I quote are five or ten trails, but then I think it is meaningful. You can observe the spread if the spread is so small.

DR. BARNES. The meteorologists usually have an extra grou t the end for special observations and provisions could be made for including an ditional in-formation.

We have now eliminated the shears and we are supplying just the wind. Next, let us consider density or perhaps decay rates.

DR. ROPER. Decay rates!

MR. MÜLLER. Decay rates!

DR. BARNES. Decay rates averaged over the same time period, one hour, starting half an hour before the hour.

MR. MYERS. Will that filter the information the same way?

DR. BARNES. Yes. Now, how about height increments? One kilometer?

MR. MYERS. With the specified accuracies.

DR. PETERSON. 1 km ± 3.

MR. MYERS. It can be changed as the equipment changes. This is a specifi-cation of your data the same way that the meteorological rocket network data changes.

DR. BARNES. But there are standard meteorological levels.

MR. MYERS. They should correspond to meteorological requirements and not with the presently realizable accuracies of the radar meteor trails.

DR. ELFORD. I wonder at this time if 1 km is a bit foolish, ± 3 or 4?

MR. MYERS. That is what we are doing, and that is what people were trying to do. They will work with other data which assigns heights to a kilometer. They may not know it as a kilometer, but they are using height to a kilometer.

DR. PETERSON. This would give you something to shoot for.

MR. MYERS. This is what is desired. I heard Wayne Hering say that they were out to have data of 1 km and we can use it to two. We can't get it now from meteor trails.

DR. BARNES. So, should we go to 1 km intervals?

MR. MYERS. I think two.

DR. BARNES. What is your pleasure, gentlemen?

MR. MÜLLER. I think 2.5 km sounds like a good component.

DR. BARNES. As far as units, do we want to specify the information at a particular interval?

DR. ELFORD. At heights of 90, 91, 92, 93, and so forth.

DR. ROPER. This doesn't really matter terribly much now.

DR. BARNES. At this point we want to look ahead so we don't get outdated.

DR. ELFORD. We have one set that can do it already.

MR. MÜLLER. The limit of this is the order of magnitude of the Fresnel zone, so this introduces a factor of 1.5 km.

MR. MYERS. Code to 1 km; that is the way I think about it.

DR. BARNES. As for units, I suggest for wind speeds, meters per second. We will use the metric system. For decay rate, decibels per second.

DR. ELFORD. We should consider wind components first. You can turn around and take any direction you like.

DR. BARNES. The meteorologists would prefer to have north-south and east-west; however, the system at Sheffield is not orientated in that direction.

MR. MÜLLER. I don't think that makes a lot of difference, though.

DR. ELFORD. You are going to specify an average of an hour. Thirty minutes of the hour in each direction?

DR. BARNES. Yes.

MR. MYERS. I think it would be foolish to do it any other way than along the cardinal axis.

DR. BARNES. This information is for the meteorologist who uses the south to north component of the wind as positive and the west to east component of the wind as positive.

DR. ROPER. So the vector is positive north and the vector is positive east, and this agrees with southerlies, which are positive, and westerlies, which are positive. So the two in fact do agree if you consider signs.

DR. REVAH. What will be the delay between the time the information is recorded and the time it is supplied to meteorologists?

DR. BARNES. If it is reduced by hand after taking it off a film, at least a couple of years.

MR. MYERS. There is some data at AFCRL which has been lying around for a couple of years.

DR. BARNES. Greenhow's data should be made available. The meteorologists are interested in how the solar cycle affects the wind, so they want to get as long a record as possible and therefore would like to pick up Greenhow's data. Now, have I overlooked anything?

DR. PETERSON. Do you really insist on having the dB or power ratio?

DR. BARNES. Would you explain?

DR. PETERSON. The decibel is a perfectly honest unit, but it is misused very frequently and it doesn't always have the same meaning. I think we can agree that it is a power ratio.

MR. MYERS. Actually, power ratio would be more confusing.

DR. PETERSON. I personally would just as soon label things in powers of ten rather than dB, but other people don't always agree, especially other engineers.

MR. MYERS. There is nothing that will confuse a meteorologist more than giving him something in dB.

DR. PETERSON. I just wonder why we can't settle on power ratio and not have to worry about it.

DR. MILLMAN. Well, a lot of astronomers get mighty confused with dB's, too.

DR. PETERSON. We could use magnitudes, too, and that is equally bad.

MR. MYERS. I think everybody understands powers of ten.

DR. BARNES. It is easy to change the program.

DR. PETERSON. You already get it in dB, and it is only a constant anyway.

DR. BARNES. I don't understand the objection.

DR. PETERSON. The objection is that there are people who somehow take the log of voltages instead of powers, and there is a conversion factor involved. They still write dB when they mean voltage ratio. It is not legal. They should square it.

MR. MÜLLER. It is silly, because the definition of dB is ever so simple.

DR. PETERSON. It is simple, but people are doing it the other way; the vlf radio people are the worst offenders. They write voltage dB and I never know what that means. It isn't that big a point.

DR. ELFORD. I don't think there is any trouble in using dB in meteorological data.

DR. PETERSON. What they will want to know is what this power ratio means.

THE FLOOR. A characteristic of time and wavelength is all you need to get density.

DR. CHAMPION. Decay time is a good quantity, though.

DR. SOUTHWORTH. It is an awkward quantity to put on a card, because when it is slow, it goes into too many columns.

MR. MYERS. Won't you only be recording decay time for short trails?

DR. ROPER. The duration depends upon the shears and whether the trail is underdense or overdense.

MR. MYERS. Well, it will go from two digits to four digits.

DR. BARNES. To summarize what we have decided on so far; one hour averages centered on the hour using Universal Time. Use U- and V-components of the wind. Plus is west to east for the U-component and V plus is from south to north. Speeds in meters per second. Standard levels every 1 km. For decay rates, I suggest

$$\frac{10}{t_2 - t_1} \, \text{Log}_{10} \left(\frac{P_2}{P_1}\right)$$

where P_i is the power in milliwatts at the time t_i.

DR. SOUTHWORTH. I don't really see the difference between specifying the log of the power ratio and ten times the power ratio; why don't you put in the wavelength and the multiplying constant and call it an apparent diffusion? Then everybody will know how it is calculated.

MR. MYERS. It would be better to specify the power.

DR. PETERSON. You better do that or at least make it an honest dB.

DR. BARNES. Is this the way we are going to agree on it? What is your pleasure, gentlemen?

All right, decay rate is given as ten times the log to the base ten of the power ratio all divided by the time interval.

Now, there is one other thing that I think we should go into concerning the information and that is the number of observations that are used to compile the three different means. You have one with U, one with V, and one for the decay rate. Don't you think that the number of observations that are used to compute, say, the U-component at a particular height should be included?

DR. ELFORD. In Dr. Revah's work, he took the east-west component at a number of heights and joined up a smooth profile through this by interpolation. I think that most of us would consider this to be a satisfactory approach, but how are you going to identify how many meteors one has in any one particular kilometer?

DR. REVAH. I would have to provide my experimental points.

DR. ROPER. And what you do with them is your business.

DR. REVAH. Yes.

DR. CHAMPION. There is no problem on this; this is done a standard way for meteorologists. At each altitude you would specify the number of measurements and this will be the altitude. We can put in a curve. That has nothing to do with the observation.

DR. ELFORD. Then you would be identifying the winds at every one kilometer.

DR. BARNES. Not every kilometer. Say that you have twenty values of U for one hour. Most will be between 90 and 95 km and in order to have meaningful averages you would have to average all those with heights between 90 and 92 km and assign this value to 91 km. Likewise the average of those in the intervals 92 to 94 km and 94 to 96 km would be assigned to 93 and 95 km, respectively. Now for other heights the intervals might have to be as much as 5 km because of the small number of observations.

FLOOR. But I thought we had agreed to 1-km intervals? Moreover, because of the variation in the height distribution of the returns and the inaccuracies in the

height determinations, I think we should adopt Dr. Elford's suggestion and take the values at each kilometer from smoothed curves the way Revah does.

DR. BARNES. There are two objections to this method. First, the data is prejudged in a subjective manner before it is given to the meteorologists. Second, one trail at a very high or very low height will have a large influence on the bottom or the top of the profile and this influence will not be known by the meteorologist using the data.

Likewise there are objections to my proposal. What is the maximum height interval that should be used? How many values should be required in a height interval?

I still am of the opinion that we should provide the usable information from each meteor on a separate card. After all, once the data are combined, or averaged, they are hard to recover. And, as far as the number of IBM cards used for the averaged data is concerned, the hourly rates would have to exceed 50 in order to bring about a significant savings.

MR. MYERS. Arnold, I think you missed the point concerning the 1-km intervals. The consensus was to average any and all reliable values assigned to a 1-km height interval. For example, one would average all V's for the interval 90.5 to 91.5 km and report this as the V for 91 km. In addition, the number of values used in each average should be reported to help the user.

DR. BARNES. Well, this is better, but if we go this far why not go all the way and give the data on each trail? After all, the noise that appears in the averages would be the signal to some investigator if he had the raw data.

MR. MYERS. At this point the feelings of the others are that they are reluctant to release the data for each trail because of possible misinterpretation by those who do not understand the problems and limitations of the radar meteor trail techniques.

DR. BARNES. To summarize:

(1) Windspeeds will be given in meters per second.

(2) West to east movements = + U.

(3) South to north movements = + V.

(4) Time will be given in Universal Time.

(5) Time averages will be for one hour starting 30 minutes before the hour and ending 30 minutes after the hour.

(6) Height intervals should be 1 km in depth centered on the integral heights.

(7) Decay rates will be given in dB/sec.

(8) For each of the time - altitude bins there may be three pairs of numbers:

    (a) average U, and number in the average;

    (b) average V, and number in the average;

    (c) average decay rate, and number in the average.

Are there any more comments?

Mr. Müller from the University of Sheffield is going to give us a paper on Atmospheric Tides in the Meteor Zone.

III. Atmospheric Tides in the Meteor Zone *

H. G. Muller
University of Sheffield
Sheffield, England

Abstract

The results of observations of upper atmospheric winds made during the IQSY by the radio meteor technique at Sheffield are described and discussed. Comparison is made with other data obtained in the Northern Hemisphere.

1. RESULTS

Wind data have been obtained in the form

$$V(t) \ = \ V_o \ + \ \sum_{i=1}^{4} \left(\sin \ \frac{2\pi i}{24} \ t \ - \ \phi_i \right) ,$$

---

*(Extended Abstract of paper given on 19th August 1966)

V(t) = horizontal wind component at time t counted in
      hours from 0-hr UT

V_o = steady prevailing wind component.

A least squares curve has been fitted by computer to each 24-hr set of data
and seasonal averages have been obtained using data for each hour and fitting a
curve to the hourly averages. The results are presented in a series of harmonic
dials (Figures 1 to 6) where a given point represents the amplitude V_i and the
phase $\phi_1 + \frac{\pi}{2}$ expressed in time of day, when the amplitude has its maximum value
in the direction specified for the dial. Prevailing wind components are treated
separately.

According to tidal theory the harmonic wind components should be represented
by vectors of constant amplitude rotating clockwise in the Northern Hemisphere.
In order to demonstrate the degree of approximation to this pattern the time scale
on the EW - harmonic dials has been advanced by 90° relative to the NS - dials.
Ideally, plots should then be found in identical positions on both dials.

In the case of the semidiurnal component, there is relatively good fit to a
tidal pattern, but seasonal changes take place and deserve further discussion.

There is little clear tendency for the two orthogonal diurnal wind components
to be 90° out of phase so that the 24-hr wind does not fit a simple tidal pattern very
well. Seasonal averages indicate that the 24-hr wind component is very stable over
long periods.

The amplitudes of the 8-hr and 6-hr components are comparatively small, and
it is difficult to establish a clear tidal pattern; but an underlying tidal mode is indi-
cated by the tendency for plots to cluster in corresponding regions of the dials. The
yearly means M_1, using all days and giving weight to each meteor echo, give a
lesser fit to a tidal mode than the means M_2 which are formed with equal weight
to each monthly average. The latter means are probably more meaningful since
the echo rate varies appreciably over the year.

Results concerning the constant term V_o in the harmonic wind analysis are
shown in Figure 7. A more detailed presentation is given in Figure 8, showing
the week to week changes of the steady, prevailing wind component. An attempt
has been made to resolve a slowly varying component of the wind system with per-
iods between 3 and 9 days.

Short term variations have been resolved during continuous wind recordings.
Some results are shown in Figure 9 and Figure 10.

The technique used in the study of general wind motions is also suitable to
resolve wind shears. Frequently, the phase variation displayed on the recorder
unit shows a pronounced acceleration or deceleration indicating an increase or

decrease in radial wind velocity. This is apparently a consequence of the reflection point moving along the meteor train whilst the orientation of the train is modified by a wind shear. Figure 11 illustrates two possible cases resulting in the same observed effect. Since the range is always decreasing with progressive tilting of the trains the velocity gradient is always positive. The observations generally support this picture, but on occasions the observed wind gradients are negative. In this case the shear cannot be linear. A possible wind profile giving rise to a negative velocity gradient is shown in Figure 12.

If we have a positive velocity gradient, the magnitude of the wind shear may be computed using the geometry of Figure 13. Then Δh follows from the equation

$$\Delta h^2 + V\Delta t \sin \theta \cos \theta \Delta h - R\Delta V\Delta t \cos^3\theta = 0$$

and $\Delta V/\Delta h$ is the magnitude of the linear shear.

A different approach to the problem of the vertical wind structure is to study the echo heights and average over velocity values applying to height groups at intervals of a few km. If the required height accuracy is not too high, echo decay rates may be used to estimate echo heights. Vertical wind profiles have been derived in this fashion at Sheffield, some results being shown on Figure 14.

It is of further interest to study the occurrence of wind shears, in particular, the diurnal and seasonal variation of the abundance of meteor echoes exhibiting wind gradients. Such data are shown in Figures 15, 16, 17, and 18.

The magnitude of observed gradients as it varies diurnally and seasonally is shown in Figures 19 and 20. Lastly, we find the distribution of vertical wind gradients in Figure 21. All results obtained over a whole year have been used in the analysis.

2. DISCUSSION

Monthly, seasonal, and yearly averages lend good support to the concept of a tidal structure in the semidiurnal wind system. Certain departures are noticeable and are illustrated by the graphs in Figure 22. Comparison with data obtained elsewhere in the Northern Hemisphere shows that the changes in phase are very regular throughout the year.

The systematic changes during the summer months suggest a solar heating effect. The harmonic dials of Figure 23, where data obtained at several locations over 12 years are included, show that the general patterns repeat each other fairly regularly year after year. This puts us in a position to sketch an idealised pattern for the seasonal distribution of amplitudes and phases (Figure 24). Such

systematic changes are inconsistent with the well-known resonance hypothesis. As an alternative, thermally excited tides have been suggested, and we have compared our results with the mode predicted by Butler and Small (1963). For the yearly averages we find very good agreement as far as the phase of the NS-semidiurnal component is concerned. Some of the departures from a simple tidal pattern may be a consequence of the existence of a standing pressure wave in the upper atmosphere. We have, in fact, some indication of such a standing wave, if we compare the respective phase of the yearly averages of the semidiurnal wind at Sheffield and Kharkov.

The 24-hr component of the wind system is generally smaller than the 12-hr component in the Northern Hemisph , and comparatively harder to resolve. A spurious 24-hr component is often introduced by slow variations of the steady prevailing component during a 24-hr period. The good definition of the seasonal averages of this component illustrate the degree of reliability of the present data. It is seen that in general, there is a tendency for the wind vector to oscillate along a straight line. On a number of occasions, we observe a nearly perfect tidal pattern for a period of about a month (Figure 25).

The higher harmonic components basically appear to conform to a tidal pattern, but departures occur quite frequently, and are probably caused by short period variations in the wind system which have recently been interpreted in terms of internal atmospheric gravity waves.

Gravity wave phenomena have characteristic horizontal and vertical scales, depending on atmospheric height, which can be resolved by the present technique. The horizontal scale follows from the ripple structure superimposed on the general tidal pattern (Figures 9 and 10), and the vertical scale can be derived from the vertical wind profiles (Figure 14). In Figure 26 we find several solid curves based on the theory of Hines, which show the relationship between the horizontal and vertical wavelengths of internal atmospheric gravity waves for the particular periods of oscillation observed on the records of Figures 9 and 10. The horizontal wavelength has been calculated for each observed mode and the respective values have been plotted on the solid curves. It is seen that these plots are all inside the permitted area of the graph where, because of the severe viscous dissipation, no reflection takes place. It is also seen that the associated theoretical vertical wavelength for all experimental points is very close to 10 km, which is in good agreement with the scales derived from our vertical wind profiles.

The results concerning the magnitude and occurrence of wind shears are at present being compared with other upper atmospheric data, but there is not enough material as yet available for a detailed discussion of these phenomena.
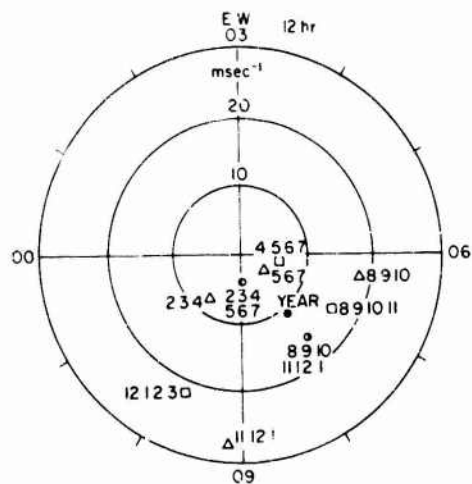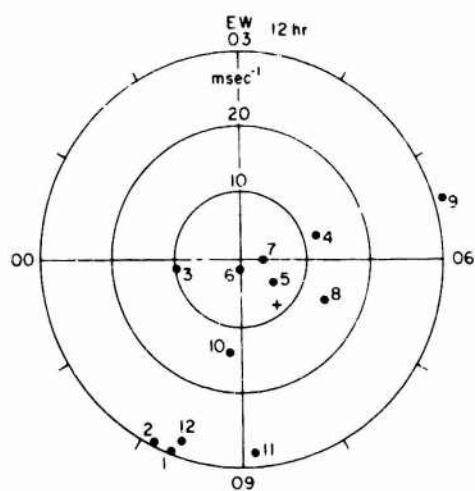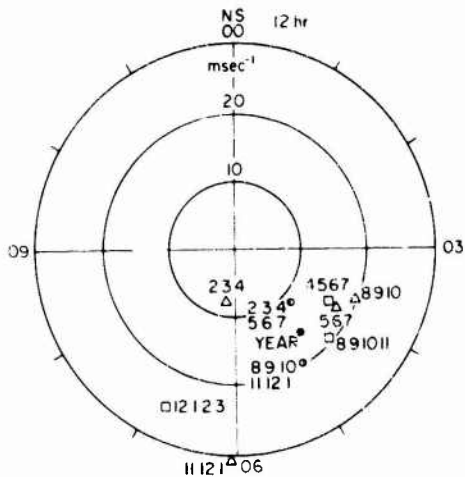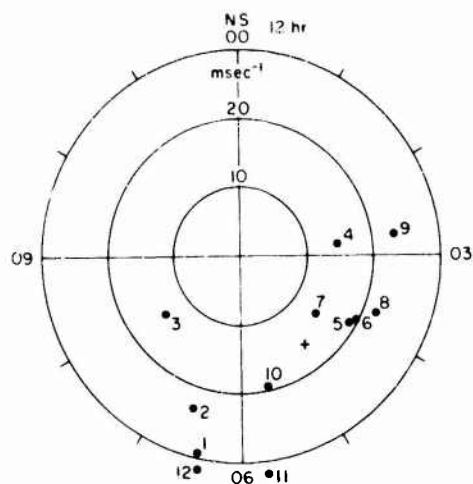
Figure 1. Harmonic Dials for the Monthly Mean NS and EW Components of the Semidiurnal Wind at Sheffield. Yearly averages indicated by +

Figure 2. Harmonic Dials for the Seasonal Mean NS and EW Components of the Semidiurnal Wind at Sheffield
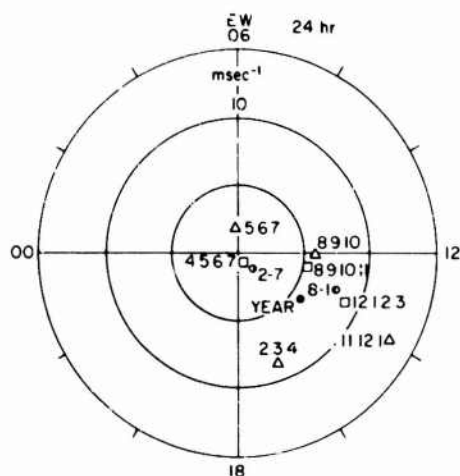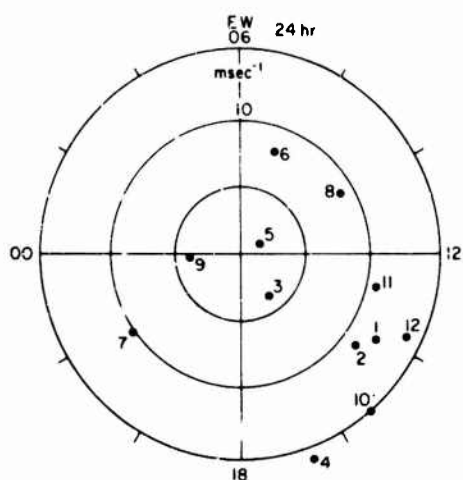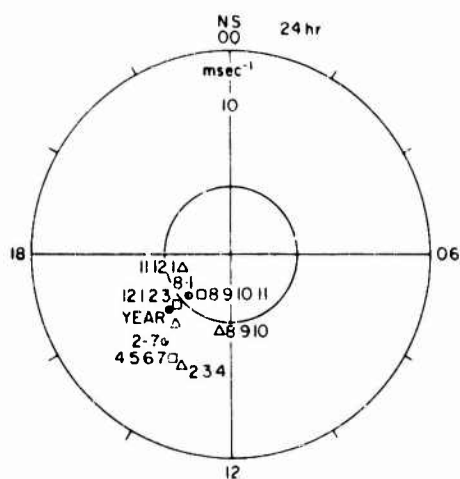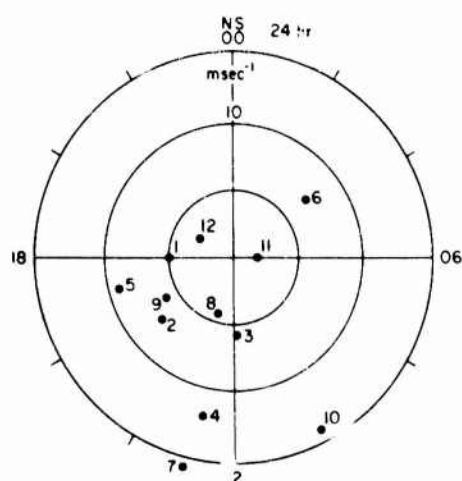
Figure 3. Harmonic Dials for the Monthly Mean NS and EW Components of the Diurnal Wind at Sheffield

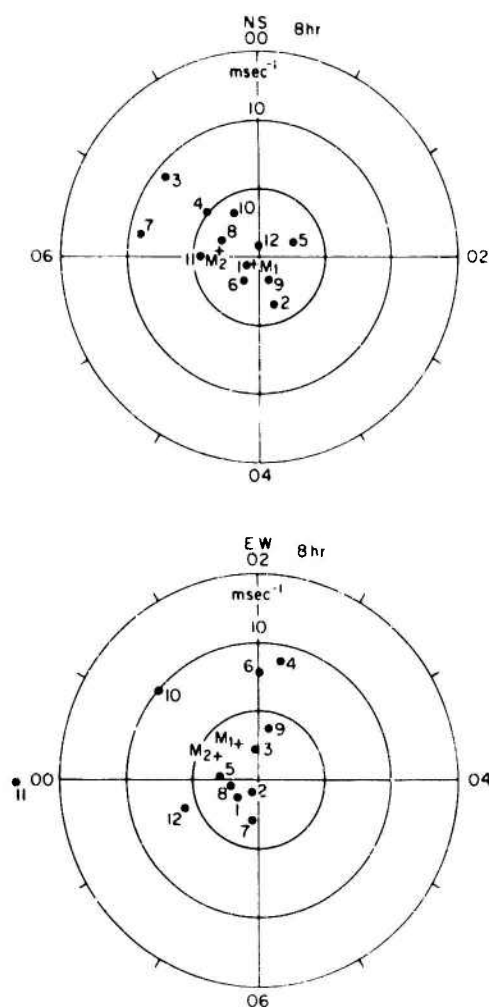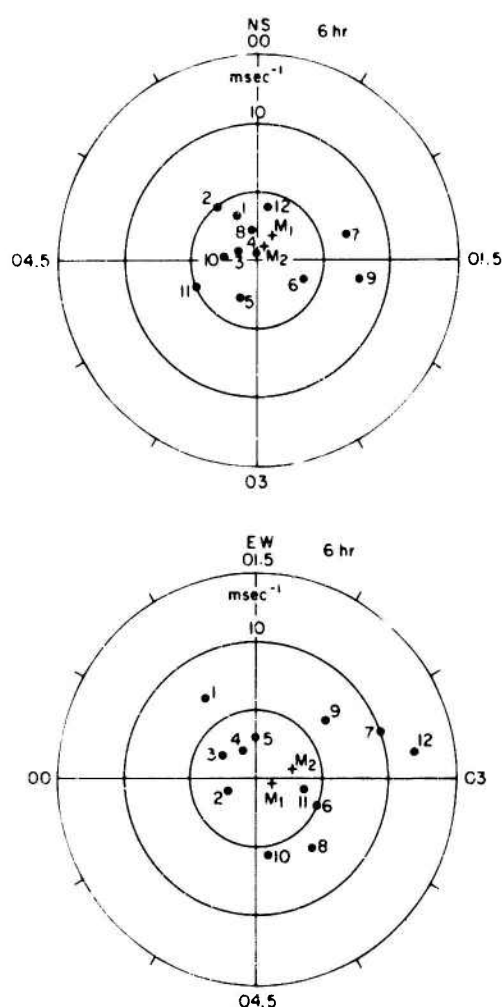Figure 4. Harmonic Dials for the Seasonal Mean NS and EW Components of Diurnal Wind at Sheffield

Figure 5. Harmonic Dials for the Monthly Mean NS and EW Components of the Terdiurnal Wind at Sheffield (for $+M_1$ and $+M_2$ see text)

Figure 6. Harmonic Dials for the Monthly Mean NS and EW Components of the Six-Hour Wind at Sheffield (for $+M_1$ and $+M_2$ see text)
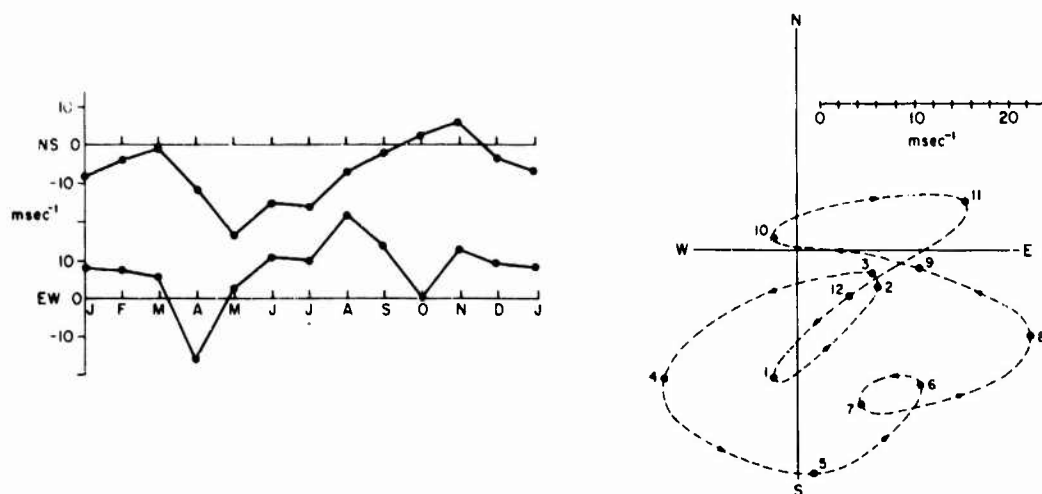
Figure 7. Monthly Averages for
the NS and EW Components and
Polar Plot of the Steady Prevailing
Wind at Sheffield
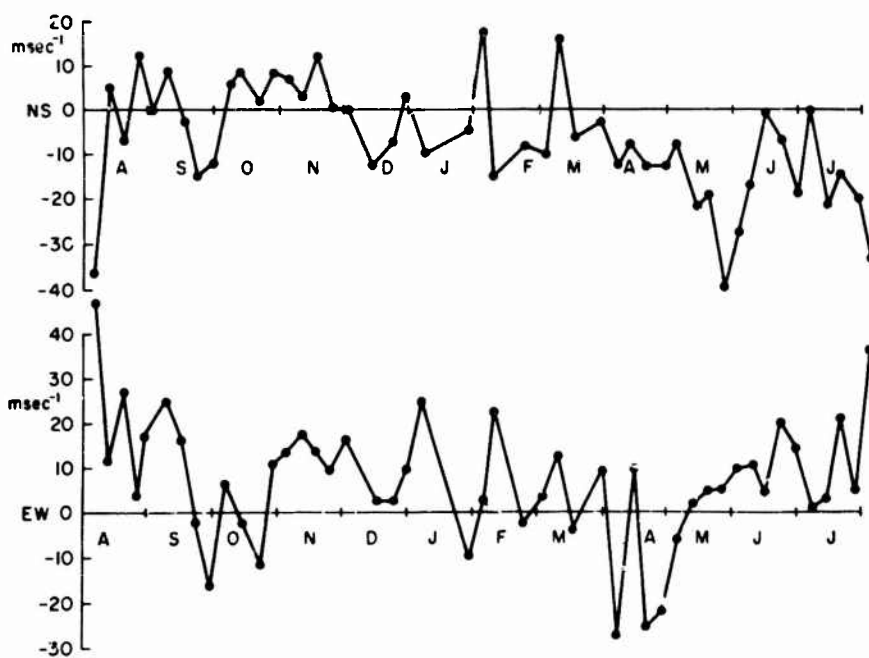
Figure 8. NS and EW Components of the Steady
Prevailing Wind at Sheffield Between August 1964
and September 1965. Results of harmonic
averages ignoring daily trends
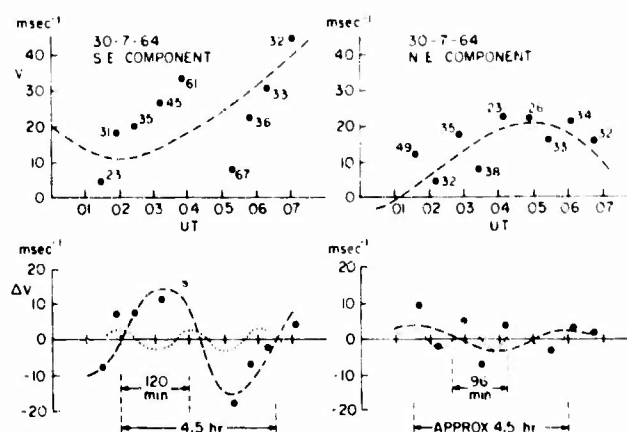
Figure 9. Short Term Variations in the Wind Pattern. Top:
Experimental values and smooth tidal curve fit (dashed line).
Bottom: Departures from smooth tidal curves showing
distinct fine structure. Numbers refer to the number of
meteors contributing to each data point
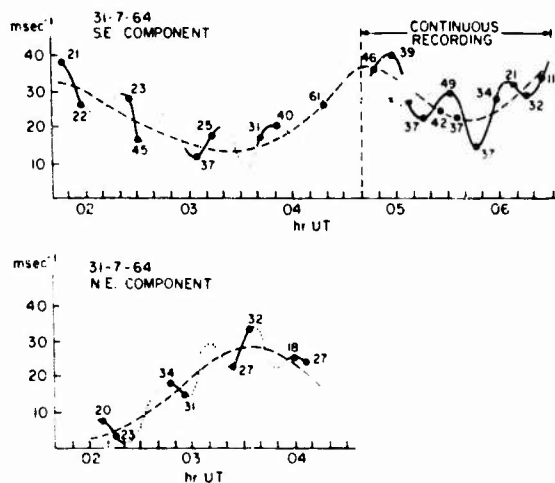
Figure 10. Short Term Variations in the Wind Pattern. The continuous recording
between 0440 and 0630 shows a significant oscillation with a period of approximately
33 min. An attempt has been made to fit a curve of similar period to the remaining
experimental points (dotted line). It is quite evident that an oscillation of a period
of the order of 2.5 hours also exists (dashed line). The smooth tidal curves are not
included in these diagrams, but reference may be made to Figure 9 for comparison
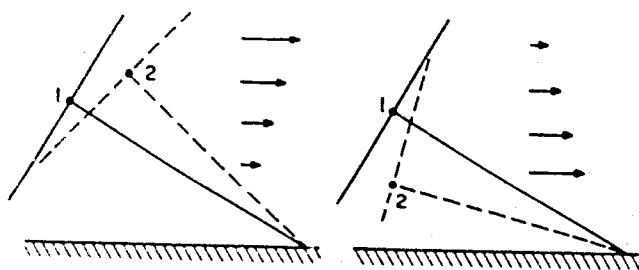
Figure 11. The Effect of a Linear
Wind Shear on the Position of the
Reflection Point on a Meteor Train
(1) t = 0, and (2) t = Δt
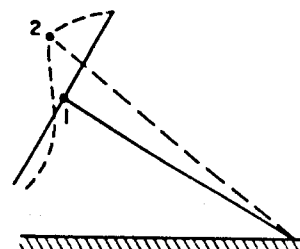
Figure 12. Distortion of
a Meteor Train by a Nonlinear
Shear
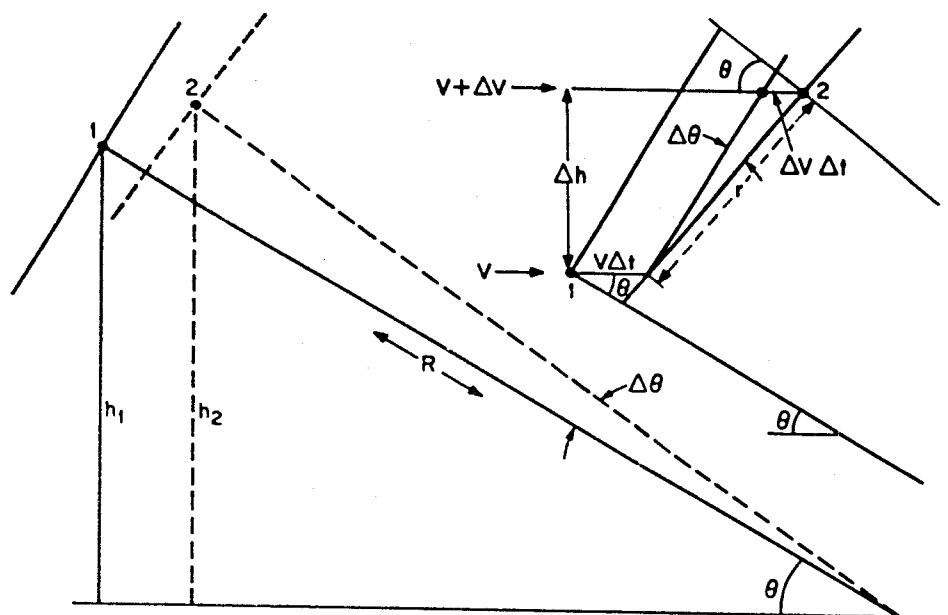
Figure 13. Simple Geometrical Model for the Evaluation of
Wind Shear Magnitudes. The meteor train is assumed to lie
in the vertical h-R-plane. Δθ may be considered small
compared with θ. Observational data are the velocity V at
time t, velocity V + ΔV at time t + Δt and the range R
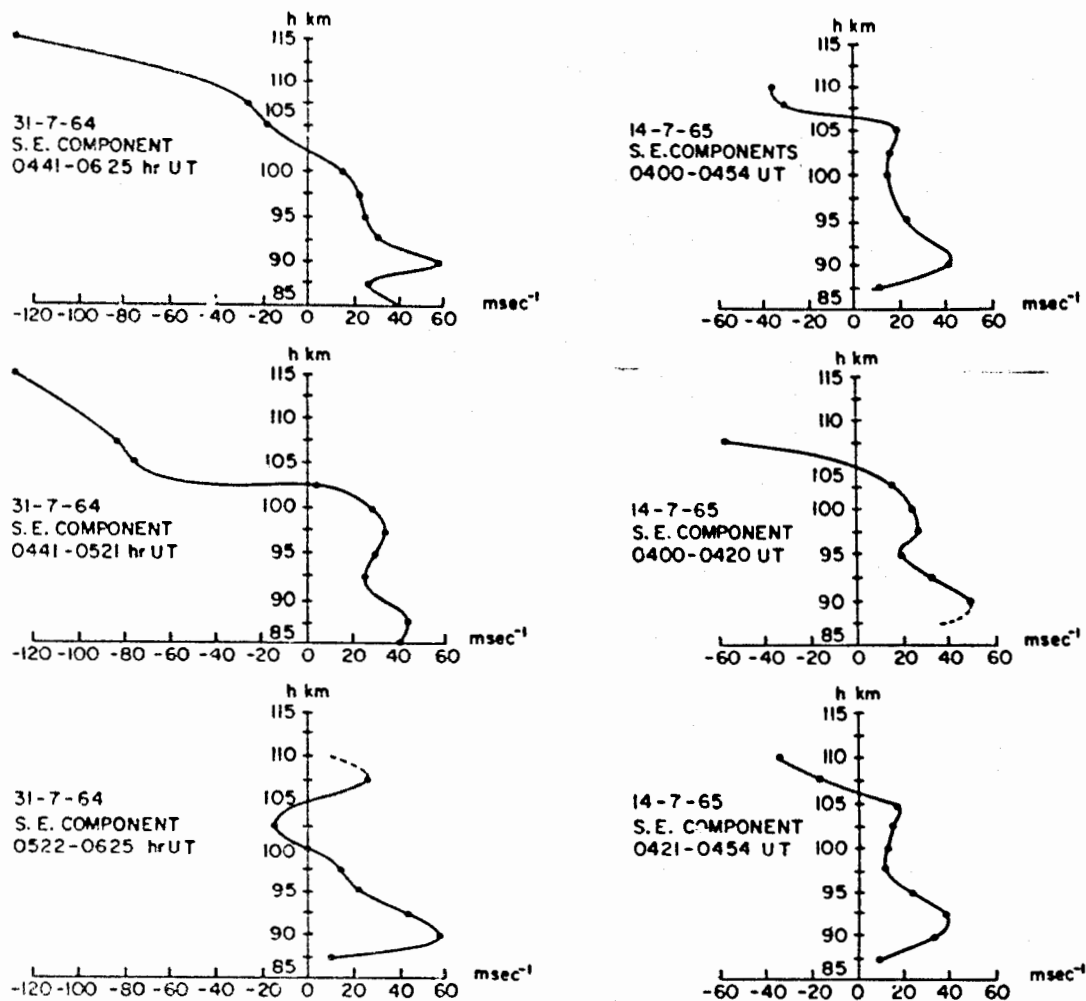
Figure 14. Height Variation of the Upper Atmospheric Wind at Sheffield on Two Days Nearly One Year Apart (results of echo decay measurements)
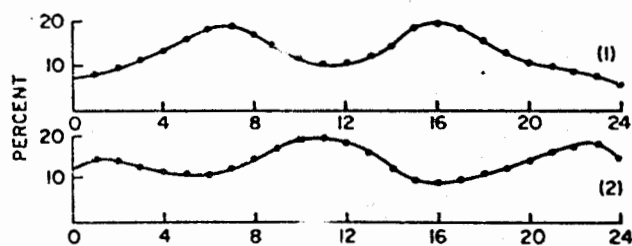
Figure 15. Relative Abundance of Meteor Echoes Showing Wind Gradients Greater Than 15 msec^{-2}. (1) September 1965, and (2) April 1965. Averages for each hour of the day
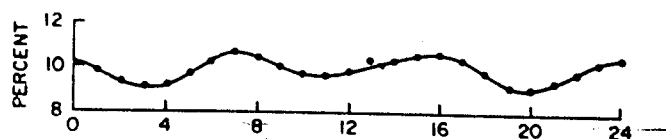
Figure 16. Relative Abundance of Echoes
as in Figure 15. Hourly values averaged
over nine months (August 1964 to May 1965)
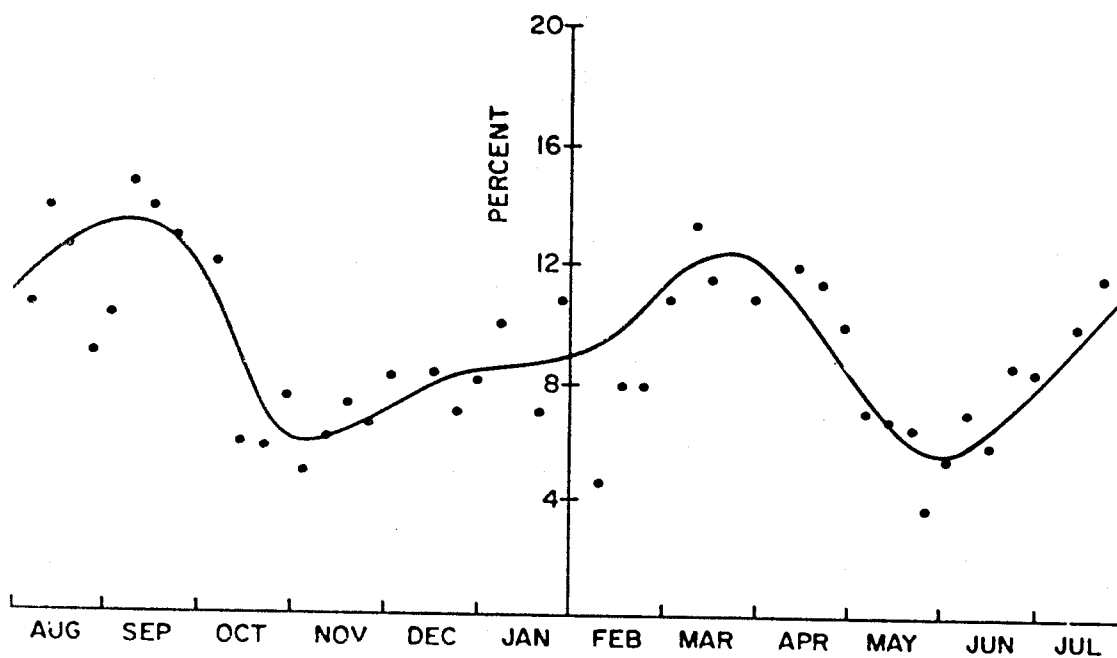
Figure 17. Variation of Daily Average Abundance
Figures Over a Period of 12 Months
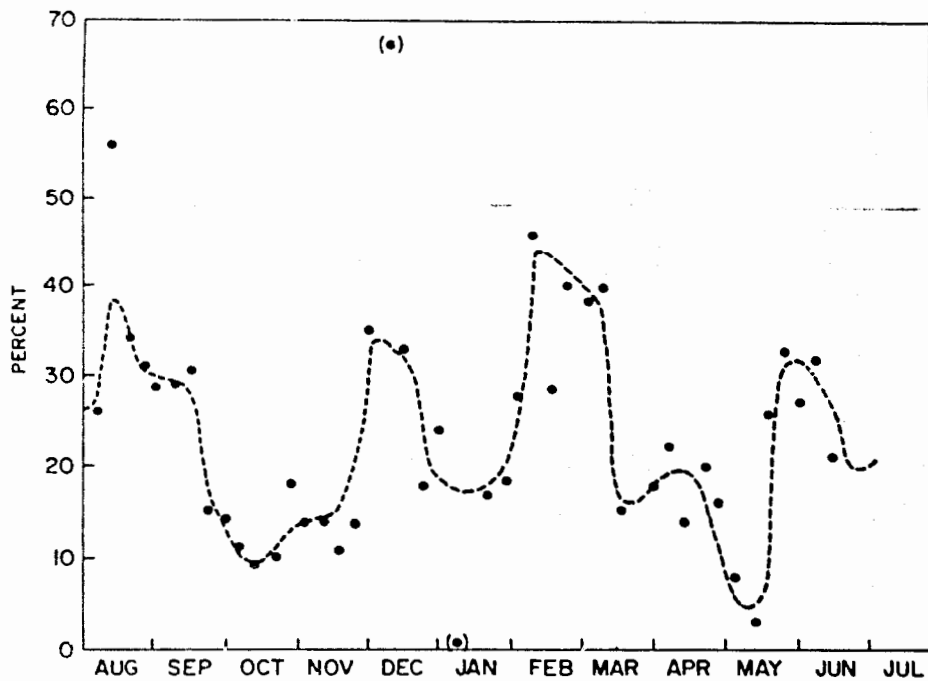
Figure 18. Twenty-four-hour Averages of the Percentage of Odd Shears Against the Total Number of Shears Observed. Dotted Curve Indicating Possible Fine Structure. NW and SW aerial results have been combined
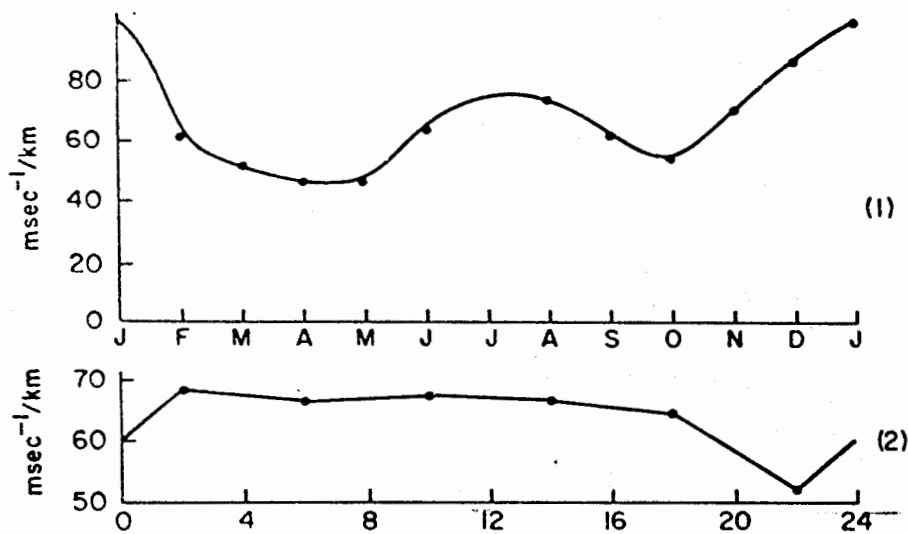

Figure 19. Monthly (1) and Hourly (2) Averages of Observed Wind Velocity Gradients Irrespective of Sign
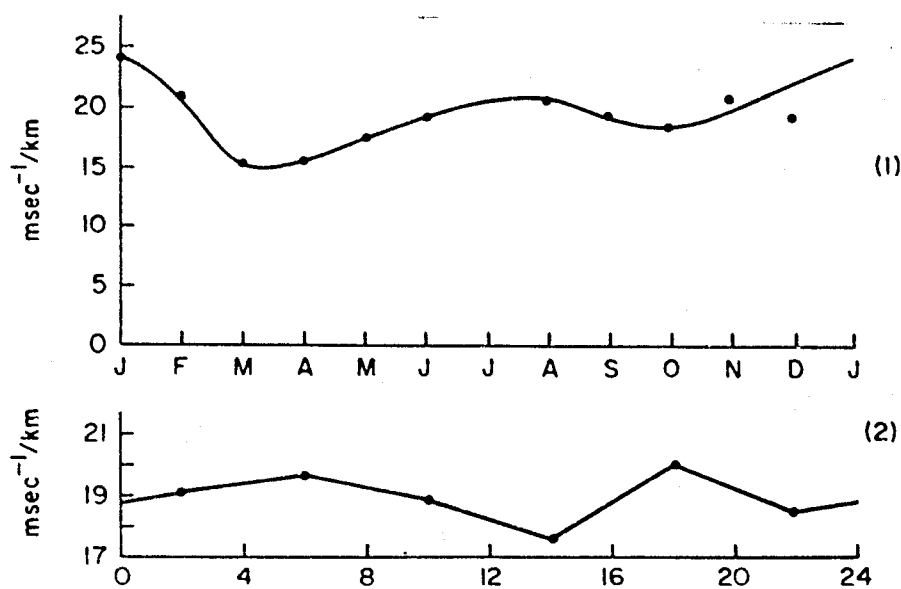
Figure 20. Monthly (1) and Hourly (2)
Averages of Vertical Wind Gradients at
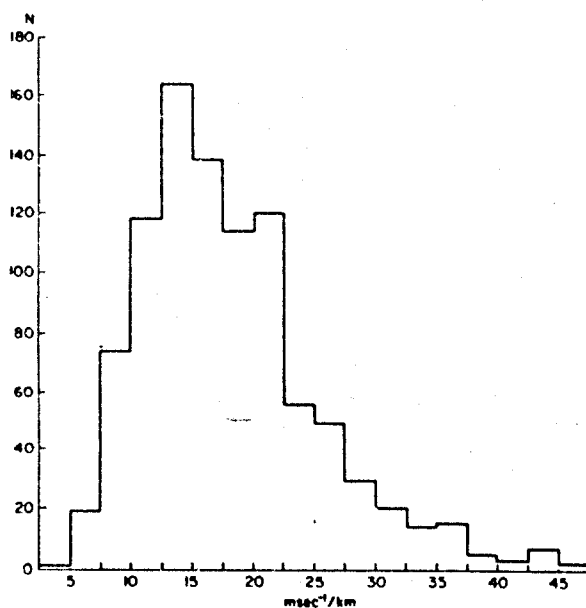Sheffield Using Results From NW and
SW Aerials

Figure 21. Distribution of Vertical Wind Gradients
Using One Year's Results. The drop in number of
gradients below 5 $msec^{-1}$/km is probably not real
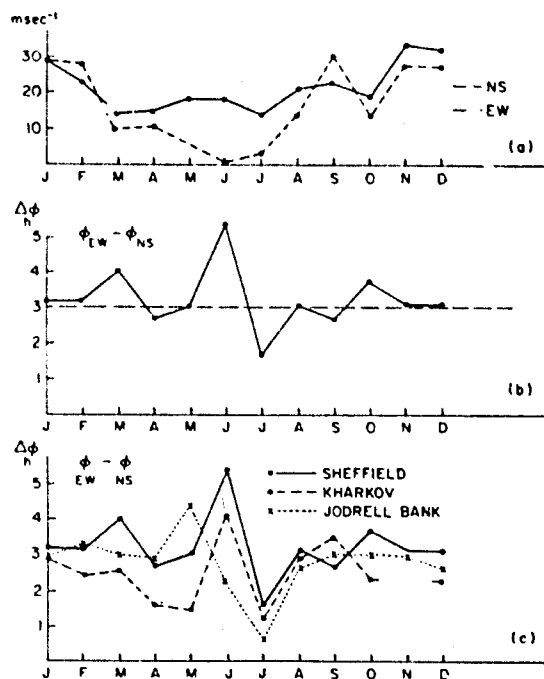but a result of selective analysis

Figure 22. (a) NS and EW Amplitudes of Monthly
Averages at Sheffield, (b) Difference Between the
Phase of the EW and NS Components, (c) Comparison
of Various Observations of Phase Difference Between
the EW and NS Components. All data applying to the
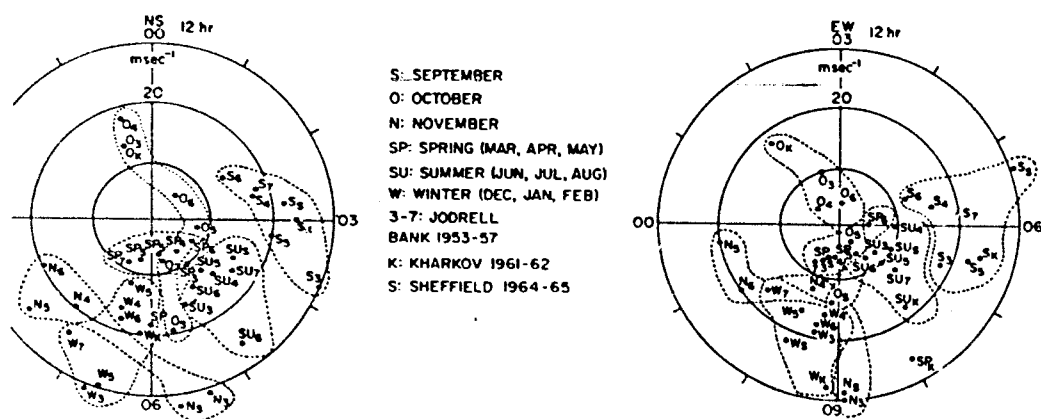12-hour wind

Figure 23. Seasonal Averages of
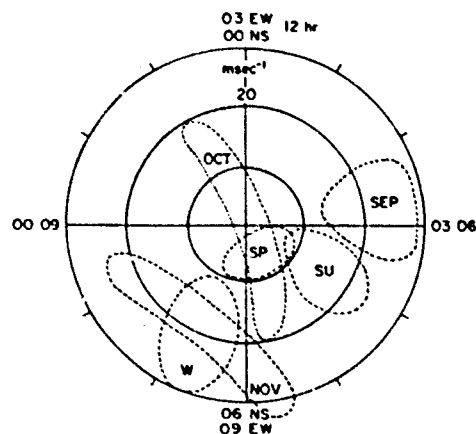the Semidiurnal Wind Over 12 Years

Figure 24. Idealised Pattern for the Seasonal Distribution of the Semidiurnal Tidal NS and EW Components Using Results From Three Stations Spread Over 12 Years (notation as in Figure 23)
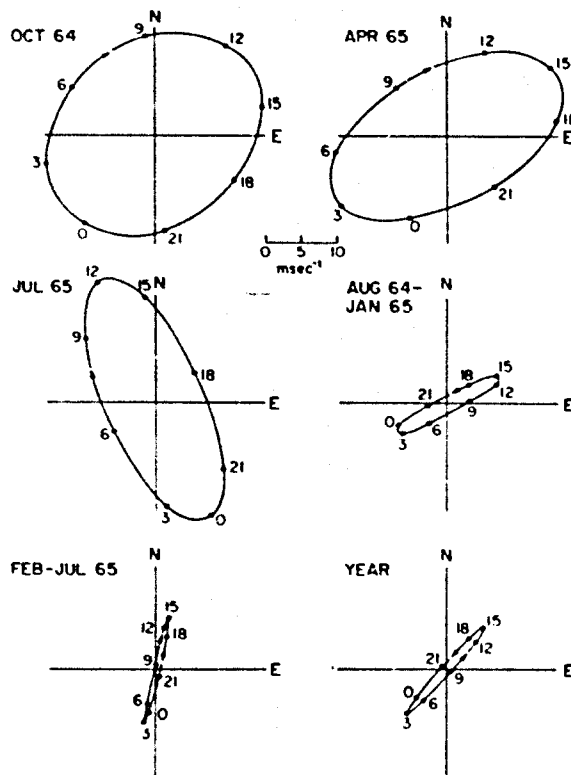

Figure 25. Polar Plots of the 24-hour Wind Component at Sheffield (numbers refer to local time)
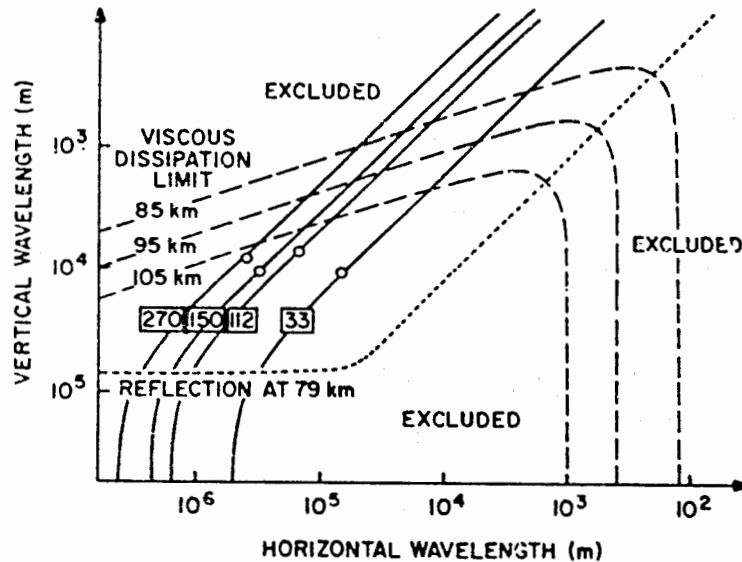
Figure 26. The Relation (solid curves) Between Horizontal and Vertical Wave-lengths for Internal Atmospheric Gravity Waves of Various Periods After Hines (1960). The figures in boxes are the periods of oscillation measured in the present work. The Sheffield data (see Figures 17 and 18) are marked by O and it is seen that the observed modes fall into the permitted region which is free from severe reflection and severe dissipation. The associated vertical wavelengths are all close to 10^4m in good agreement with the values derived from the (unpublished) vertical wind profiles obtained at Sheffield for the periods of observation

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Butler, T.S., and Small, K.Z. (1963) Proc. Roy. Soc. A., 274: 91-121.

Greenhow, J.S. and Neufeld, E.L. (1955) Phil. Mag., 46: 549-562, Proc. Phys. Soc. (1960) 75: 228, Quart. J.R. Met. Soc. (1961) 87: 472-489.

Hines, C.O., (1960) Canad. J. Phys., 38: 1441.

Lysenko, I.A., (1963) Astron. Zhurnal, 40: 161-170.

Müller, H.G., Atmospheric tides in the Meteor Zone, Planetary Space Sci. (in press).

DISCUSSION

DR. BARNES. Is there anyone who has any questions?

DR. ELFORD. Have you investigated the height profile as a function of time of day?

MR. MÜLLER. No. We have only done very sporadic measurements. The height profile requires continuous runs, and we find that this is rather difficult. For instance, within two hours, we expose 100 feet of 35-mm film, which corresponds to a half day's records. We can do this only on certain occasions. I have done it, now, since the first of August. So, there may be something coming out. We were separating the components over a period of about two hours, but I cannot say anything about diurnal variations. I cannot even resolve a 12-hour component.

DR. ELFORD. Have you investigated the height distribution as a function of time?

MR. MÜLLER. The height distribution of what?

DR. ELFORD. Just the echoes of your reflection.

MR. MÜLLER. No, the same applies there. We have the height distribution for the times when we were running high numbers, and this was usually early in the morning, so that this is between let's say midnight and eight o'clock in the morning.

DR. ELFORD. I would like to make a comment on this. If you refer to that slide of Revah's you might recall that I made a comment that there was a very obvious variation in the mean height of the echoes. Now, this can have a very severe effect on the phases that you get in your periodic components.

MR. MÜLLER. Yes. I do agree. This is sort of an overall picture that we get. You have to recast the data and pick individual heights in order to make it worthwhile. I think that at this stage we can only say that we are carrying on very

simple lines as Greenhow did very many years ago. Variations might show up. We can compare IGY results with IQSY. At this stage we seemed to be far more advanced to be able to assign heights to each sort of mode. I can't do this yet.

DR. ELFORD. You have to be careful not to inject the phase variation at a given level and have a height variation that does not fit.

MR. MÜLLER. That is right. I think that what we said before about the variation in echo heights and numbers throughout the day should be considered.

MR. MYERS. It isn't only the numbers; it is also the distribution of height that can mess up your whole thing.

MR. MÜLLER. Yes, this is easy enough. This we can get.

DR. BARNES. Any more questions? Thank you, Mr. Müller. Do we have any more comments? Is there anything that anybody would like to discuss?

DR. ELFORD. I mentioned this to Arnold, and I think that it might be reiterated. Astronomers know enough about the rate distribution of the sporadic meteors so that one can predict the expected echo rate as a function of time of day for any given antenna system for any given wavelength. This is what, if calculated, would be called the response function of the system.

DR. PETERSON. To what accuracies?

DR. ELFORD. Oh, I would think about 20 percent.

DR. PETERSON. I just looked up some data, and that is why I am asking. On a half million echoes, the standard deviation seemed to be more like 50 percent.

DR. ELFORD. The purpose is not just to get the numbers, but to suggest that you might be able to reorient your antenna beams to try and maximize the minimum values that you get. You might be able to use more than your two beams. You might have several pairs of beams that you use.

DR. PETERSON. This report, which is certainly not new, from Mlodnosky of Stanford shows the hourly rates on a three year period with rotating antenna. This was 1960, but I am pretty sure that we have copies of this report. As I say, he looked at some of the variations over this period and the deviation from the mean values on the omni-directional basis. He also plotted azimuth-range plots (which are then the projection of the radar plane) of the radar gradient distribution, and some of them show very marked peaks. The only problem is that there are some that don't repeat every year.

DR. ELFORD. I think that you find that you get excellent information for 15 hours, but then for the other 9 hours you are really struggling. Had you done this beforehand, you might have been able to make some improvements in observational methods to elevate the numbers in those 9 hours.

DR. PETERSON. This was more apparent at times from the oblique communication studies that we did where you had almost zero rates unless you programmed your antenna.

DR. MILLMAN. There is no doubt about it that the regular main pattern repeats year after year. We get the main pattern on our patrol. I think that Graham's suggestion of directing antennas so that you do corral more meteors for wind studies is a good one. There is no reason why that shouldn't improve it.

DR. ELLYETT. That information is available in the southern and northern hemispheres.

DR. BARNES. This reflects on something that we did earlier this morning. Say you are looking in two directions that are 90 degrees apart, so that you are obtaining two 90-degree components of the winds. In order to get the U and V, you have to combine them, and I am wondering if we should go back and perhaps put in the two directions rather than, say, U and V? Otherwise you will have to combine your data at a particular height.

DR. PETERSON. You do anyway. It is only a question of getting two projections. All we are suggesting is that instead of looking north and west, you look northwest and so forth to maximize the number of returns.

DR. CHAMPION. You perhaps might put a footnote on the card in which direction you are looking, something like that.

DR. PETERSON. The rates, of course, unless you happen to be at the pole or at the equator, vary quite drastically, so you ought to pick them carefully. Any other points?

DR. ELFORD. I have, in fact, a program that is available. If you can supply your antenna patterns and geographical location, I can give you some rough predictions of echo rates for any day of the year for any given antenna direction.

MR. MYERS. Wouldn't it be better to supply a list of preferred antenna directions?

DR. ELFORD. Well, you have to start off with your beam width.

DR. PETERSON. You can do the inverse problem and pick beams that will help you, but it isn't going to work.

DR. BARNES. Anyone who would like to visit the AFCRL Radar Meteor Trail trailer to see what we have in the way of equipment can ride with me and we can have lunch up there at the CRL cafeteria.

I want to thank the attendees for coming to this Workshop, and I especially want to thank our foreign visitors from Australia, France, Great Britain, and Canada. Even though Dr. Barad is not here, I would like to express my thanks to him for the personal interest that he has shown in this Conference. Thank you gentlemen.

Appendix A

LIST OF REGISTRANTS

Mr. Kenneth Baker
NASA Manned
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Figure A1. Participants at Workshop: Front row, left to right: Dr. W. G. Elford, University of Adelaide; Prof. C. D. Ellyett, University of Newcastle, Australia; Dr. P. M. Millman, National Research Council of Canada; Dr. M. L. Barad, AFCRL; Dr. A. A. Barnes, Jr., AFCRL; Dr. C. S. Nilsson, NASA; H. G. Muller, University of Sheffield; Dr. I. Revah, CNET, France; Maj. T. A. Studer, AWS; Prof. A. D. Frost, University of New Hampshire.

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DOCUMENT CONTROL DATA - R&D

(Security classification of title, body of abstract and indexing annotation must be entered when the overall report is classified)

1. ORIGINATING ACTIVITY (Corporate author)	2a. REPORT SECURITY CLASSIFICATION
Air Force Cambridge Research Laboratories (CRH) L. G. Hanscom Field Bedford, Massachusetts 01730	UNCLASSIFIED 2b. GROUP

3. REPORT TITLE

PROCEEDINGS OF THE WORKSHOP ON METHODS OF OBTAINING WINDS AND DENSITIES FROM RADAR METEOR TRAIL RETURNS

4. DESCRIPTIVE NOTES *(Type of report and inclusive dates)*

Scientific.   Interim.

5. AUTHOR(S) *(First name, middle initial, last name)*

Editors,
Arnold A. Barnes, Jr.
Joseph J. Pazniokas

6. REPORT DATE	7a. TOTAL NO. OF PAGES	7b. NO. OF REFS
April 1968	331	20

8a. CONTRACT OR GRANT NO. b. PROJECT, TASK, WORK UNIT NOS. 8628-08-01 c. DOD ELEMENT 6240539F d. DOD SUBELEMENT 681000	9a. ORIGINATOR'S REPORT NUMBER(S) AFCRL-68-0228 9b. OTHER REPORT NO(S) *(Any other numbers that may be assigned this report)* Special Reports No. 75

10. DISTRIBUTION STATEMENT

Distribution of this document is unlimited. It may be released to the Clearinghouse, Department of Commerce, for sale to the general public.

11. SUPPLEMENTARY NOTES TECH, OTHER	12. SPONSORING MILITARY ACTIVITY Air Force Cambridge Research Laboratories (CRH) L. G. Hanscom Field Bedford, Massachusetts 01730

13. ABSTRACT

The first day of the four-day meeting held 16-19 August 1966 was devoted to technical descriptions of six radar meteor trail systems. Methods of deriving winds, wind shears and geometric height of the trails were presented on the second day. Discussions of ambipolar diffusion rates and derived atmospheric densities and density-heights were the topics for the third day. On the last day the discussion centered around the use of the data by the meteorologist. The height resolution and data rates needed for climatological, tidal and turbulence studies were delineated. Two papers on wind studies at Sheffield, England and at Adelaide, Australia were presented.

14. KEY WORDS	LINK A		LINK B		LINK C	
	ROLE	WT	ROLE	WT	ROLE	WT
Meteor trails						
Radar meteor trail returns						
High altitude winds and densities						
Mesospheric winds and densities						